



U.S. Department  
of Transportation

**National Highway  
Traffic Safety  
Administration**

# **Review of the Literature Evaluating the Effect of Countermeasures to Reduce Alcohol Impaired Driving (1980–1989)**

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**Final Report  
Volume I—Synthesis**

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16. Abstract  This two-volume report documents the results of an extensive review and analysis of impact evaluations of alcohol-traffic crash countermeasures. Evaluations published since 1980 are covered by the review. Volume I presents a synthesis of the findings on the impact of three major classes of countermeasures: (1) restricting alcohol availability, (2) deterring and incapacitating drunk drivers, and (3) treating and rehabilitating drunk drivers. Volume II contains summaries of the assessments of individual evaluations.					
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- Herbert M. Simpson, Traffic Injury Research Foundation of Canada;
- Evelyn R. Vingilis, Addiction Research Foundation;
- Robert B. Voas, National Public Service Research Institute, Inc.; and
- Allan F. Williams, Insurance Institute for Highway Safety.

This project required an-depth review of the evaluation studies that were identified during the course of the project. Drs. Perrine, Vingilis, Voas, and Williams assisted us in this review process, as did Drs. Donald W. Reinfurt and J. Richard Stewart of The University of North Carolina Highway Safety Research Center, and Dr. Hans C. Joksch of Mid-America. We appreciate all the work our reviewers put into this effort.

When we began this project, we contacted a number of researchers and practitioners in the alcohol safety field and in related fields and asked them to help us identify pertinent literature. So many responded that we do not have room to list them all here, but we are grateful for their help.

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Ralph K. Jones

John H. Lacey

## CONTENTS

ACKNOWLEDGEMENT .....	i
CONTENTS .....	ii
EXECUTIVE SUMMARY .....	iii
CHAPTER 1 - INTRODUCTION .....	1
CHAPTER 2 - REGULATING ALCOHOL AVAILABILITY .....	5
RAISING THE LEGAL MINIMUM DRINKING AGE .....	5
RESTRICTING ALCOHOL SALES .....	11
INCREASING TAXES ON ALCOHOLIC BEVERAGES .....	13
SUMMARY AND CONCLUSIONS .....	14
CHAPTER 3 - DETERRENCE AND INCAPACITATION .....	18
ENFORCEMENT .....	18
ADJUDICATION .....	25
SANCTIONS .....	27
<i>Drivers License</i> .....	27
<i>Jail</i> .....	38
<i>Probation</i> .....	39
<i>Other Sanction Evaluations</i> .....	40
COMPREHENSIVE APPROACHES .....	41
SUMMARY AND CONCLUSIONS .....	50
CHAPTER 4 - TREATMENT AND REHABILITATION .....	58
DISCUSSION .....	58
SUMMARY AND CONCLUSIONS .....	63
CHAPTER 5 - CONCLUSIONS AND RECOMMENDATIONS .....	65
BIBLIOGRAPHY OF CITED DOCUMENTS .....	68
BIBLIOGRAPHY OF EVALUATION REVIEWS .....	77

## EXECUTIVE SUMMARY

The objective of this project was to perform a comprehensive assessment of the traffic safety *impact* of drunk driving countermeasures in the United States. In addition, some related countermeasures implemented in other countries (particularly Canada and Australia) were examined to provide perspective or to fill gaps in U.S. evaluations.

Pertinent documents published during the period 1980-1989 were screened and reviewed by the authors and by a distinguished group of experts in the field of alcohol and traffic safety. The review process involved an assessment of each study individually, and comparisons of studies of similar countermeasures across the states. The analysis of countermeasures across states sought to identify not only studies with positive and significant effects, but also those with no significant or even negative effects. Three generic types of countermeasures were eventually identified:

- Regulating the availability of alcohol;
- Deterring and incapacitating drunk drivers; and
- Treating and rehabilitating drunk drivers.

With respect to *regulating the availability of alcohol*, we found that the evaluations of countermeasures that raised the legal minimum drinking age (LMDA) include many that are among the best in the highway safety field. These evaluations clearly show that controlling the availability of alcohol by raising the LMDA can markedly reduce alcohol-related fatalities. We estimate that the reduction in alcohol-related fatal crashes among the affected age group due to raising the LMDA has been on the order of 9% to 14%. However, there is no clear evidence of a spillover effect into other age groups.

Other attempts at controlling the availability of alcohol have rarely been evaluated to determine their traffic safety effect. Such countermeasures include restricting the sales of alcohol (for example, banning happy hours, and having self-testers in bars for determining blood alcohol concentration) and placing increased taxes on alcoholic beverages. The few evaluations of countermeasures in the former group found that none of them had any significant highway safety impact, either directly or indirectly. The evaluations of the latter group were plagued by methodologic difficulties, but did suggest that raising the cost of beverage alcohol through increased taxes may have potential.

Countermeasures aimed at *deterring or incapacitating drunk drivers* use various elements of the Traffic Law System to catch and punish drunk drivers, thereby

creating a perception that drunk drivers are at risk of being sanctioned. There is evidence that countermeasures that have stressed *enforcement* can have a significant traffic safety deterrent impact, particularly when used in combination with a strong public information and education (PI&E) component. Some of the stronger studies of the effects of enforcement coupled with public information campaigns found reductions in the number of nighttime accidents ranging from 10 to 30%. However, some other strong studies found only small or "possible," but not significant, effects. Thus, the amount of reduction in alcohol-related crashes achieved by enforcement countermeasures is difficult to quantify. Enforcement strategies employing the concept of sobriety checkpoints appear to have been successful in Australia and France, and some limited research in the U.S. suggests they may have been an important factor in some DWI programs that have combined enforcement with enforcement-strategy specific PI&E.

One *adjudicative* countermeasure aimed at increasing the probability that a charged drunk driver will be convicted of drunk driving (implied consent) was found also to have a traffic safety benefit in itself by suspending refusers' drivers licenses. Another adjudicative sanction, deferring prosecution as an incentive for entering a treatment program, was found to be ineffective.

Of evaluated countermeasures focussing on *sanctions*, those that suspended or revoked a DWI's driver license are clearly the most effective, particularly when well-publicized and applied administratively. One strong study showed that suspending the license of drivers refusing to submit to an alcohol test reduced their accident involvement during suspension, including alcohol-related accidents by about 70%, presumably because most did not drive while under suspension. Several other studies of different degrees of strength showed that suspending or revoking licenses for DWI reduced all accidents as well as alcohol-related accidents during the period of suspension or revocation.

The impact of other sanction-directed countermeasures remains unclear. For example, one statewide study of incarceration in a jail reported no effect, while another local study of the same sanction reported a positive effect. In fact, there is still considerable controversy about the effect of sanction severity on drunk driving. Three studies reviewed here (Ross, 1987; Ross and Voas, 1989; and Joksch, 1988) suggest that the *severity* of a sanction may be less important than the *certainty* of a sanction, while another study (Zador *et al.*, 1988) found that certain severe sanctions (including jail) were highly effective.

Several studies have evaluated *comprehensive programs* employing multiple countermeasures. The results of these evaluations have been mixed. For example, the evaluation of the STOP-DWI program in New York state found that nighttime crashes fell by 27% during a four-year period while the program was operating, while daytime crashes dropped by seven percent. A study of changes in Kansas's DWI law and another study of changes in Minnesota's DWI law also found a positive effect statewide. On the other hand, a strong evaluation of changes in California's DWI laws concluded that the 12.9% statistically significant reduction in fatal crashes

computed by his model were not due to the law but were more likely due to "some unknown factor." Evaluations of law changes in North Dakota, Oregon, and Washington indicated no significant effect. None of the evaluations of statewide programs used other states as a control, leaving claims of effectiveness somewhat questionable.

The evaluations reviewed in this study do not provide strong support for the hypothesis that alcohol-related crashes can be reduced by *treatment and rehabilitation*, although two strong studies found reductions in the re-arrest rate ranging from 10 to 35%. This conclusion applies to programs that deal with social drinkers and first offenders as well as to programs that deal with persons with drinking problems and with multiple offenders. Further, there appears to be a disturbing tendency for the better designed and executed evaluations to show little or no impact, and for the less rigorous evaluations to show an impact. Nevertheless, more recent studies continue to confirm past studies indicating that rehabilitative sanctions can be effective when applied in addition to traditional sanctions such as driver's license suspension or revocation.

The reader should keep in mind that the period during which many of the countermeasures examined here were in effect or evaluated included the economic recession of 1982. Although it is well established that the economy has a strong effect on traffic deaths, the quantitative relation is not well understood. It is very difficult to control for economic factors in an explicit way, and to control for them implicitly may not be reliable either. For example, control states may have different economic trends, and control groups of accidents, such as daytime accidents, may be affected differently by a recession (it appears that daytime accidents are more affected than nighttime accidents). Further, control groups of drivers, if not assigned randomly, may come from different socio-economic groups and be differentially affected by the recession. Therefore, if a countermeasure was implemented during or shortly before the recession (as many of the countermeasures examined here were), or if data from the recession period were used as baseline data, an evaluation might be affected. Under such conditions, *a change might erroneously be ascribed to the countermeasure, or a real effect of the countermeasure might be masked*. Therefore, the reader should exercise care (as we have tried to do) in interpreting the results of any evaluation of a countermeasure using data from the period 1981 through 1984.

Synopses of our reviews of the impact evaluations discussed in this volume are provided in tabular form at the end of Chapters 2, 3, and 4, and are also reproduced below in this Executive Summary for ready reference. The entries in the tables are listed alphabetically by author. Our own comments on study findings are enclosed in brackets.

Summary of Evaluations of Countermeasures Restricting the Availability of Alcohol<sup>a</sup>

Reference	Description	Design	Findings
Blose and Holder, 1987	A law permitting sales of liquor by the drink in several North Carolina counties.	1,3,4	The program did not increase alcohol-related accidents, suggesting a counter-program would not decrease accidents.
Brown and Maghsoodloo, 1981.	Lowering the LMDA in Alabama.	2	Increase in alcohol-related single-vehicle crashes after LMDA lowered (19 to 21).
Colon and Cutter, 1983	Effect of beer consumption and other availability variables on fatal accidents in the 50 states and DC.	2	This cross-sectional analysis found a significant correlation between fatal accidents and beer consumption. [Finding weakened by the research design used.]
DuMouchel, Williams, and Zador, 1986	Raising the LMDA in 26 states.	2,3,4	Nighttime fatal crashes reduced by 13% overall, 10% for males and 26% for females.
Hoskin, Yalung-Mathews, and Carraro, 1986	Raising the LMDA in 10 states.	1,4	90% of affected age group had crash reductions, compared to 30% of unaffected age group.
Hoxie and Skinner, 1987	Analyzed data from 51 states over a 10-year period to find effect of raising the LMDA.	1,3,4	A 9%-13% reduction in fatal crash involvements for affected age group. No spillover effect on other age groups.
Hoxie and Skinner, 1989	Update of their 1987 study using more controls.	1,3,4	A 10%-13% reduction in fatalities and no spillover effect on other age groups.
Lacey, <i>et al.</i> , 1989	Raising the LMDA in North Carolina.	1,4	Reductions in alcohol-related crashes for the affected age group.
McKinnon and Woodward, 1986	Raising the LMDA in three states.	1,3	Positive effects in at least two of the three states studied.
Phelps, 1985	Examined the relationship between alcohol taxes and fatal traffic accidents.	NA	Found that an increase in the price of alcohol would decrease alcohol consumption. [The numerical amounts of the price elasticities are not plausible.]
Russ, Geller, and Leland, 1989	A review of evaluations of breathtesters placed in bars to provide impairment feedback to customers.	NA	Studies in the U.S., Canada, and New Zealand found that the devices were generally ineffective and may even have increased alcohol consumption in some instances.

Reference	Description	Design	Findings
Smart and Adlaf, 1986	A law banning happy hours in Ontario, Canada.	2	No effect found. [There were many confounding factors that were not accounted for.]
Smart and Adlaf, 1987 <sup>b</sup>	Age of majority cards issued young persons in Ontario, Canada.	NA	The cards were found to be ineffective and possibly counterproductive.
U.S. General Accounting Office, 1987	Synthesis of prior studies on raising the LMDA.	NA	Raising the LMDA generally reduces alcohol-related crashes for affected age groups. Effect was 5%-28% in four sound studies.
Wagenaar, 1982	Effect of lowering and then raising the LMDA on beer and wine consumption in Michigan.	1,4	Decrease in total beer consumption of 7%. Concurrent new bottle deposit law may have contributed to effect of LMDA.
Wagenaar, 1983	Raising the LMDA in Michigan and Maine.	1,3,4	20% of all alcohol-related young-driver crashes can be prevented by removing access to alcoholic beverages.
Walsh, 1987	Studied the effect of various variables related to alcohol consumption on traffic deaths per vehicle in Ireland.	2	Found that traffic deaths decreased with decreasing consumption. [Result is not conclusive because of approach used (regression) did not adequately account for possible "other factors."]
Williams <i>et al.</i> , 1983	Raising the LMDA in nine states.	1,3,4	For target groups of the laws, fatal crashes reduced by 14%-23%, depending on type of fatal crash.
Womble, 1989	Raising the LMDA in 13 states.	2,4	Estimated a 12% reduction in fatal crash involvements per licensed driver for potentially affected drivers.

a. Codes for research design of general deterrence component are: 1 - time series 2 - other, non-time series; 3 - control jurisdictions; 4 - other controls.

b. Not an impact evaluation. Data based on survey responses.

Summary of Evaluations of Enforcement Countermeasures<sup>a</sup>

Reference	Description	Design	Findings
Calderwood and Woods, 1983	BATmobiles in Albuquerque, NM.	1,3,4	Concluded that the program reduced accidents, but data were insufficient for predicting the amount of the reduction.
Homel <i>et al.</i> , 1988	Random breath tests with a PI&E program in New South Wales, Australia.	2	Found a 33% reduction in number of fatally injured drivers with a BAC of .05% or more.
Lacey <i>et al.</i> , 1986	A large-scale, multi-year enforcement / PI&E program in two adjacent FL cities.	1,3,4	Found a significant intervention effect of 13%-20% on nighttime crashes.
Lacey <i>et al.</i> , 1988	Similar program to the previous study. Test site was Indianapolis, IN.	1,3,4	Found only a possible effect due to a less intense PI&E campaign.
Levy, Shea, and Asch, 1989.	Sobriety checkpoints combined with an education campaign in New Jersey.	1,4	Found that the checkpoints decreased nighttime single-vehicle crashes by 10%-15%.
Lynn, 1985	13 selective enforcement programs in VA.	2	Ten of the 13 localities met their stated goals in terms of alcohol-related accidents. [Study not conclusive because of lack of controls.]
McLean, 1984	Random breath tests in South Australia.	Roadside Surveys	Found a 14% reduction in percentage of drivers at or above a BAC of .08%. Effect disappeared within a year.
Pigman and Agent, 1986.	A combined enforcement / PI&E program in Lexington-Fayette County, KY.	2	Found a 30% reduction in reported alcohol-related accidents due to the program. [Some effect is likely, but may be less than found because of possible time trend.]
Ross, 1987b	A one-month combined enforcement / PI&E program in England and Wales.	2	Found a 23% reduction in all fatalities during the month the program was in effect. [Apparent lack of a control groups makes finding questionable.]

Reference	Description	Design	Findings
Vingilis, Chung, and Adlaf, 1981	A two-year combined enforcement / PI&E program in the Toronto, Canada area.	1,3,4	Found no measurable highway safety effect.
Voas, Rhodentizer, and Lynn, 1985.	A one-year sobriety checkpoint operation in Stockton, CA.	1,3,4	Found a significant 15% decrease in alcohol-related accidents in Stockton. Decrease was not significant when compared with decrease in the rest of the state.
Voas and Hause, 1987	Increased patrol activity in Stockton, CA.	1,3,4	Found significant 10%-15% decreases in nighttime accidents due to the countermeasure.
Wolfe and O'Day, 1984	A 4-year combined enforcement / PI&E program in Oakland County, MI.	2	Alcohol-related accidents decreased, but no control groups were reported. [Thus, the actual effect of the program is unknown.]

a. Codes for research design are: 1 - time series; 2 - other, non-time series; 3 - control jurisdictions; 4 - other controls.

Summary of Evaluations of Adjudication and Sanctioning Countermeasures<sup>a</sup>

Reference	Description	Design	Findings
Amick and Marshall, 1984	Comprehensive DWI program in Bonneville County, ID.	1,3,4	A significant decrease in nighttime injury crashes.
Blomberg, Preusser, and Ulmer, 1987; Preusser, Blomberg, and Ulmer, 1988	Wisconsin's law mandating 3 to 6 month driver-license suspensions for 1st offense DWI.	1,4,7,9	A 25% reduction in alcohol-crashes (from general deterrence analysis). 50% reduction in subsequent DWIs after first six months of program (specific deterrence analysis).
California State Department of Motor Vehicles, 1986	Warning letters and informational materials sent to DWI first offenders in California.	5,6,9	Warning letters were not effective in reducing subsequent DWIs or accidents.
Eavy, Edwards, and Lee-Gosselin, 1987	A Michigan program requiring a group re-examination for beginning probationary drivers with two or more traffic convictions.	5,9	Fewer subsequent convictions and accidents among the experimental group.
Falkowski, 1984	Mandatory jail policy in Hennepin County, MN.	1,3,4	A 20% reduction nighttime injury crashes due to the policy.
Hagge and Marsh, 1988	California's provisional licensing program for age 16-17 drivers.	1,3,4,9	General deterrence component found a positive effect of the program on all accidents, but no significant effect on alcohol crashes. Specific deterrence component found no effect on subsequent accidents or convictions.
Hagen, McConnell, and Williams, 1980	Driver license suspensions in California.	6	Suspensions are effective in reducing subsequent accidents and DWIs involving first and multiple offenders.
Haque and Cameron, 1987	Victoria, Australia law prohibiting learner and probationary drivers from driving with a BAC > 0.	1,4	A 4% non-significant reduction in serious casualty accidents among the target group due to the law. There were insufficient data to show an effect of less than 10%.

Reference	Description	Design	Findings
Helander, 1986b	Habitual traffic offender law in California (charge rates and conviction rates).	NA	Because of concern for double jeopardy, prosecutors only charged 4% of those meeting HTO criteria. Only 1% were convicted.
Hilton, 1983	Changes in California's DWI law (See Helander, 1986a).	1,4	No effect compared to non-alcohol crash surrogates
Institute for Traffic Safety Management and Research, 1985a,....,1985d; Dowling, 1986; McCartt and Dowling, 1985	New York State's comprehensive STOP-DWI program.	2,4	Program generally showed an effect overall and several various counties where it was implemented. Nighttime crashes dropped 27% compared to 7% for daytime crashes in New York.
Joksch, 1988	The effect of severe sanctions in 7 states that had laws requiring them.	1,3,4	No effect of severe sanctions on fatal crashes involving drivers with a high BAC.
Jones, 1985	Oregon's comprehensive DWI law.	1,4	Non-significant decrease in alcohol-related traffic deaths.
Jones, 1986	Driver license revocation for habitual offenders in Oregon.	9	Revoked drivers had lower subsequent high-risk traffic violations (including DWI).
Jones <i>et al.</i> , 1988	Mandatory jail law in Tennessee.	1,3,4, 6,7,9	No significant reduction in alcohol-crash surrogates. Temporary reduction of 11% in reduction of DWI recidivism.
Kadell and Peck, 1982	Re-examination of California drivers with two or more major violations (including DWI) with possible driver license action.	5,6,9	Significant reduction in reconvictions due to the program. The program was deemed probably cost-effective.
Klingberg <i>et al.</i> , 1984; Salzberg and Paulsrude, 1984	Washington state's comprehensive DWI law.	6,9	A non-significant <i>increase</i> in subsequent alcohol-related accidents for experimental and control groups. [Lack of adequate controls may have contributed to this negative result.]
Lacey <i>et al.</i> , 1989	Adoption and awareness of an administrative <i>per se</i> law in Nevada.	1,4	A 10% reduction in nighttime crashes and a further 7% after the law was publicized.

Reference	Description	Design	Findings
Marsh, 1987	California's habitual offender program called the Negligent Operator Program.	5,6,8,9	Program effect was not significant for the DWI subgroup, but was positive and significant for the HTO subjects overall.
McKnight, Hyle, and Albrecht, 1983	Curfew law in Maryland.	1,4	No significant reduction in nighttime accidents among target group (compared to 25% for study by Preusser <i>et al.</i> ).
Minnesota House of Representatives Research Department, 1985	Minnesota's comprehensive DWI law.	1,3,4	An 18% reduction in the number of fatalities in the state due to the law.
Neff <i>et al.</i> , 1983	Probation versus rehabilitation in Mississippi.	5,9	No significant effects of the sanctions for problem drinkers. Rehabilitation alone not effective for problem drinkers or non-problem drinkers.
Preusser <i>et al.</i> , 1983	Curfew laws in four states.	2,3,4	Laws reduced reported crashes among target group during curfew by 25%-69%.
Ross, 1987a	New Mexico's administrative <i>per se</i> law.	1,4	A 10% decline in the percentage of drivers and pedestrians with a BAC of .05% or more.
Ross and Voas, 1989	The effect of severe sanctions in a small town in Ohio.	Roadside surveys, 3,7,8	No general deterrence or specific deterrence effect. Had small sample size.
Sadler, 1986	California's implied consent law which required driver license suspension for refusing a BAC test.	6	Suspending driver license of breath test refusers can be an effective countermeasure. Suspended refusers had 64% fewer alcohol-related crashes than non-suspended refusers.
Sadler and Perrine, 1984	Comparison of license suspension with treatment for multiple offenders in California.	6,7,8,9	Treatment group had 9% fewer alcohol-related convictions than suspension group, but suspension group and treatment group had the same number of alcohol-related crashes.
Salzberg and Klingberg, 1981	Driver license revocation for male habitual offenders in Washington state.	9	License revocation is an effective countermeasure, and there was no evidence that stay of revocation or treatment was effective.

Reference	Description	Design	Findings
Salzberg and Klingberg, 1986	Washington state's deferred prosecution law allowing charge dismissal after completion of treatment.	6	No positive effects of the program, and a possible negative effect.
Shore and Maguin, 1988	Kansas's comprehensive DWI law.	1,4	A 20% reduction in fatal accidents due to the law. [Similar reduction in Nebraska.]
Tashima and Peck, 1986	Comparison of license suspension and license-restriction-plus-treatment in California.	6,9	Suspension more effective than restriction-plus-treatment for multiple offenders. For 1st offenders, treatment more effective in preventing alcohol-related crashes; suspension more effective for all crashes.
U.S. DOT, NHTSA, 1987	North Dakota's DWI law whose main feature was administrative <i>per se</i> .	1,4	A non-significant decrease in various alcohol-crash surrogates.
Vingilis <i>et al.</i> , 1988	License suspension and random spot checks in Canada.	2,3,4	A small, short-term effect was attributed to the program.
Votey, 1984	Overall legal-system control actions in Sweden and Norway.	1,2,4	Increases in alcohol consumption are associated with higher accident levels. Increasing certainty of sanctions will reduce fatal and serious accidents.
Williams, Hagen, and McConnell, 1984	Driver license suspensions in California.	9	1st offender DWIs without suspensions had generally higher recidivism than multiple offenders with suspensions.
Zador <i>et al.</i> , 1988a; 1988b	The effect of severe sanctions in all of the contiguous 48 states that had them.	1,3,4	2.4% reduction in fatal crashes for administrative <i>per se</i> ; 4.6% for license suspension; 2.2% for mandatory jail or community service.

- a. Codes for general deterrence research design are: 1 - time series; 2 - other, non-time series; 3 - control jurisdictions; 4 - other controls. Codes for specific deterrence research design are: 5 - random assignment to treatment and control; 6 - Non-random assignment, covariance analysis; 7 - other non-random assignment; 8 - control jurisdictions; 9 - other controls.

Summary of Evaluations of Treatment and Rehabilitation Countermeasures<sup>a,b</sup>

Reference	Description	Design	Findings
Blount, 1983	DWI schools and group therapy in two counties in Florida.	5,9	35% reduction in recidivism for social drinkers sent to DWI school. Recidivism of problem drinkers given school / treatment 60% lower than control's. Only 55% of test groups finished course.
Holden, 1983	Various combinations of probation, therapy, education, and supervision in Memphis TN.	5,9	None of the treatments had any significant effect, either for social drinkers or problem drinkers.
LeClair, Felici, and Klotzbier, 1987	Prison confinement for the treatment of multiple offenders.	7	Experimental group had a lower recidivism rate than those not assigned to the program. [Assignment was selective.]
Neff <i>et al.</i> , 1983	Probation, rehabilitation, and probation plus rehabilitation in Mississippi.	5,9	The various interventions had no effect on DWI recidivism.
Popkin, Stewart, and Lacey, 1988	DWI schools in North Carolina.	6	Small effect on recidivism; no effect on crashes.
Reis, 1982	Education program for first offenders in Sacramento, CA.	5,9	Reduced DWI recidivism 2-3 percentage points, but had no significant effect of accidents because of the small number of accidents incurred by the group.
Stewart <i>et al.</i> , 1987	Several education and counseling programs in California.	5,9	No differential effects among various approaches studied. Possible decline in drinking-driving for all approaches.
Temer <i>et al.</i> , 1987	A 1-year education and treatment program (including AA and the use of Disulfiram where indicated).	6	Program completers had a lower recidivism than non-completers. No difference between AA and Disulfiram groups. [Possibly confounded by lack of controls.]

- a. Codes for specific deterrence research design are: 5 - random assignment to treatment and control; 6 - Non-random assignment, covariance analysis; 7 - other non-random assignment; 8 - control jurisdictions; 9 - other controls.
- b. Two cites from the summary table on adjudication and sanctioning also apply to this table. They are Sadler and Perrine (1984) (page xii) and Tashima and Peck (1986) (page xiii).

## CHAPTER 1 - INTRODUCTION

This report presents a synthesis of the results of a research project performed by Mid-America Research Institute for the National Highway Traffic Safety Administration (NHTSA) under contract number DTNH22-88-C-05126, entitled "Historical Documentation and Assessment of Existing Literature in the Field of Alcohol Traffic Safety Evaluation (1980-Present)." Mid-America was joined in this effort by the University of North Carolina Highway Safety Research Center (HSRC).

The objective of the project was to perform a comprehensive assessment of the traffic safety *impact* of drunk driving countermeasures in the United States.<sup>1</sup> In addition, some related countermeasures implemented in other countries (particularly Canada and Australia) were examined to provide perspective or to fill gaps in U.S. evaluations. The assessment covers documents published during the period 1980-1989.

The general approach taken involved the following substantive tasks:

- Collection, initial review, and screening of impact evaluation materials to identify those that would be incorporated into the project's final report.
- Analysis and assessment of these materials on a state-by-state basis to identify those programs which have proven successful in reducing motor vehicle crashes and related disutility.
- Collection, analysis, and assessment of pertinent unpublished information, and integration of this information into the assessment conducted under Task 2.
- Preparation of a "library" of studies containing acceptable impact evaluation literature published during the period 1980-1989, and the findings of Mid-America's review and assessment of each study.
- Preparation of a technical report containing the results of the research conducted in prior tasks.

This volume summarizes the results of the last two tasks. An accompanying volume contains the individual analyses and assessments.

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<sup>1</sup> Impact evaluations are those that deal with the effect of a countermeasure on traffic safety, rather than just the effect on variables that are believed to be related to traffic safety. We have departed from this rule only occasionally, to illustrate a particular point.

The materials that were obtained and screened were scientific reports describing impact evaluations of alcohol-safety countermeasures. Sources included collections and individual documents that had not been placed in traditional collections. Types of repositories that were contacted included:

- Specialized libraries of highway safety literature maintained by such organizations as NHTSA, the University of North Carolina Highway Safety Research Center, The University of Michigan Transportation Research Institute, and the Insurance Institute for Highway Safety;
- Specialized computerized information services such as TRIS (and its highway transportation subfile, HRIS), MEDLARS, MEDLINE, and EMBASE;
- Specialized information clearinghouses and abstracting services such as NIAAA; Johns Hopkins; Alcohol, Drugs, and Driving: Abstracts and Reviews; Addiction Research Foundation; Alcohol, Drugs and Traffic Safety: Current Research Literature; and Alcohol Epidemiologic Database;
- General libraries having collections in related disciplines such as medicine, law, and the social sciences; and
- General repositories and information services maintained by governmental agencies (e.g., NTIS, Library of Congress).

Four general categories of materials were surveyed: books, journals, reports, and conference proceedings.

Nearly all of the documents were in the last three categories. The starting point in the search was recent bibliographies and reviews of directly related materials. Other relevant bibliographies and reviews were identified through a search of the HSRC library, and through discussions with our advisory panel and others.

The next step in the search was to examine methodically specific journals and conference proceedings known by the principal investigators to contain pertinent materials. These documents were not necessarily concerned directly with highway safety, but tended to focus on other related disciplines such as human factors, toxicology, and alcohol studies in general.

We used several approaches to getting outside input in identifying source materials. First, we formed a project advisory panel of persons with both broad and specialized expertise in the field of alcohol safety program evaluation and related areas. The panel also provided guidance on the assessment itself, and also provided suggestions for improving it. A second mechanism for identifying relevant evaluations was the professional committees and societies whose members contribute to the scientific literature. We also obtained input from NHTSA and from contractor staff not directly involved in the project. Input from NHTSA was especially important because many of the evaluations turned out to be sponsored by state agencies funded by the 402 program which requires an evaluation component. Some other agencies of the federal government have sponsored evaluative research in the field. These

agencies include the National Institute of Alcoholism and Alcohol Abuse and the National Institute of Justice and were also contacted in the course of our literature search effort.

Not all of the documents that were collected were suitable for use in our assessment. Documents were not selected for review until they had been read and screened by the principal investigators. There were two primary screening criteria for substantive materials. First, a document had to address pertinent topics as it appeared to when it was identified as a candidate. For example, a document whose title and key words indicated that it discussed a new set of data on the effect of administrative license revocation on alcohol-crash risk, could have been found to be a process evaluation not concerned with traffic safety impact, and thus not pertinent to this update. Second, a document must at least have purported to have scientific validity. Documents merely reflecting the unsupported opinions of their author were not retained for review.

Documents surviving this initial screening underwent further screening to determine whether they really did have scientific validity, that is, whether the methods used in designing and executing the research, and in analyzing the results were sound. Further, we attempted to determine whether the treatment of the results was objective and balanced. However, documents that were flawed in some respect were not necessarily rejected in this screening. For example, some studies that were well-designed and executed, but made conclusions that did not flow from its findings were retained, with our commentary noting the inconsistencies between the research results and the conclusions, and offering a more consistent interpretation of the results.

The literature we collected and screened was then critically reviewed and assessed by the project staff and other reviewers to identify those programs that appeared to have reduced traffic crashes and related deaths, injuries, and other losses. This review process involved two major steps:

- An assessment of each study individually, and
- Comparisons of studies of similar countermeasures across the states.

Each study was reviewed individually with respect to the following aspects:

- The nature of the program that was evaluated, the problem addressed by the evaluation, and overall methodology followed in the evaluation;
- The population included in the study;
- The analytic framework and procedures followed, including the research design and statistical methods used; and
- The results of the evaluation, including the inferences and conclusions drawn from the study.

The analysis of countermeasures across states sought to identify not only studies with positive and significant effects, but also those with no significant, or even negative, effects. Three generic types of countermeasures were eventually identified:

- Regulating the availability of alcohol;
- Deterring and incapacitating drunk drivers; and
- Treating and rehabilitating drunk drivers.

Each of these three countermeasure types is the subject of a separate chapter of this report. The nature and findings of these evaluations is summarized in a table at the end of each of these chapters. References are provided at the end of this volume. Note that this report *does not cite every document contained in the individual analyses and assessments*. Only those evaluations that the project's reviewers found to be acceptable by the above criteria are discussed here. Reviews of other evaluations can be found in Volume II of this report.

In making his or her own assessment of the findings of these evaluations, the reader should keep in mind that the period during which many of the countermeasures were in effect or evaluated included the economic recession of 1982. Traffic deaths declined in 1981 and 1982, levelled off in 1983, and started a slow rise in 1984. Fifteen states introduced *per se* laws in 1983, 11 in 1982 or 1984. Ten states introduced administrative license suspension in 1983, and 12 states introduced mandatory jail or community service for first offenders in 1982 or 1983 (Zador *et al.*, 1988). Nineteen states raised their legal minimum drinking age in 1981 through 1984 (Hoxie and Skinner, 1989). Although it is well established that the economy has a strong effect on traffic deaths, the quantitative relation is not well understood. It is very difficult to control for economic factors in an explicit way, and to control for them implicitly may not be reliable either. For example, control states may have different economic trends, and control groups of accidents, such as daytime accidents, may be affected differently by a recession (it appears that daytime accidents are more affected than nighttime accidents). Further, control groups of drivers, if not assigned randomly, may come from different socio-economic groups and be differentially affected by the recession. Therefore, if a countermeasure was implemented during or shortly before the recession, or if data from the recession period were used as baseline data, an evaluation might be affected. Under such conditions, *a change might erroneously be ascribed to the countermeasure, or a real effect of the countermeasure might be masked*. Therefore, the reader should exercise care (as we have tried to do) in interpreting the results of any evaluation of a countermeasure using data from the period 1981 through 1984.

## CHAPTER 2 - REGULATING ALCOHOL AVAILABILITY

This class of countermeasures uses the legal system to control access to alcoholic beverages. A number of initiatives have been suggested for accomplishing this, but only a few have been assessed scientifically. This chapter treats acceptable evaluations found and reviewed in this project. These evaluations fall into three major groups, raising the minimum legal drinking age, restricting alcohol sales, and increasing taxes on alcoholic beverages.

### RAISING THE LEGAL MINIMUM DRINKING AGE

Raising the legal minimum drinking age (LMDA) has been by far the most frequently introduced and studied availability measure. A number of strong evaluations of the LMDA have been conducted dating back to the early 1980s. In July, 1984, a federal law was signed by the President and passed requiring all of the states in the U.S. to adopt a minimum drinking age of 21 by September, 1986, or forfeit a portion of their federal highway funds. By 1988 all of the states had adopted a minimum drinking age of 21.

Wagenaar (1983) gives detailed results of his studies of the experience of Michigan and Maine (which raised the drinking age from 18 to 21 in 1978, and from 18 to 20 in 1977, respectively) that used New York and Pennsylvania as comparison states. The author provides a very detailed, excellent description of his research design. In general, the design was a sound one, employing a quasi-experimental time-series design with comparison series. His statistical methods employed the Box-Jenkins approach and ARIMA models. From the Michigan findings on property damage crashes and injury crashes, the study concluded that "20% of all alcohol-related crashes involving young drivers can be prevented by removing access to alcoholic beverages" (p. 101). The Maine study showed an effect only for property damage crashes.

About the same time, Williams *et al.* (1983) reported the results of their study of nine states that raised their LMDA from 18 to 19, 18 to 20, 18 to 21, and 19 to 21 between 1977 and 1980. The population studied was drivers from ages 15 through 20. Drivers aged 21 were included in control groups. In addition to the nine states with the raised LMDA, nine nearby states that did not raise their LMDA were used as control states.

The research design was essentially a  $2 \times 2 \times 2 \times 2$  table: accidents more/less affected by alcohol  $\times$  before/after law change  $\times$  affected/not affected age group  $\times$

treatment/control state. Two techniques were used to distinguish those accidents more affected by alcohol, and those less affected by alcohol: nighttime versus daytime, or nighttime single vehicle versus daytime multi-vehicle fatal accidents. The authors also presented data for "all types" of fatal accidents (without describing how that analysis was performed). Treatment groups were the age groups directly affected by the law. As control groups, drivers of older age, up to 21, not affected by the law change were used. The authors also mention briefly results for drivers younger than those affected by the law change. The statistical approach was essentially testing the logarithms of the odds ratio in the multi-variate table. The data used in the analysis were from NHTSA's Fatal Accident Reporting System (FARS) for the years 1975-1980.

Williams *et al.* developed three sets of estimates for the net reductions in fatal crashes in the law-change states among the drivers targeted by the laws: nighttime, 23%; single-vehicle nighttime, 25%; and all types, 14%.

In a carefully designed and executed study, DuMouchel, Williams, and Zador (1986) examined the traffic safety impact of raising the LMDA in 26 states during the years 1977-1984, comparing the experience of those states with that of 22 states that did not raise their LMDA. The study population was composed of drivers age 16 to 24. The authors used two statistical approaches in their analysis, the first using a weighted regression procedure to fit a model expressing the number of fatal crashes as a function of driver age, year, and state. Crash data were taken from NHTSA's Fatal Accident Reporting System (FARS) for the years 1975-1984. The second approach studied pseudo-cohorts of drivers, e.g., those 16 years old in 1975, 17 years in 1976, etc., until 21 years in 1984. For each state, six such cohorts were formed. Data used in the regression analysis were pooled for the cohorts, and the totals were examined.

In the first analysis approach, DuMouchel and associates found a  $13\% \pm 5\%$  (95% confidence interval) reduction in nighttime fatal crashes, and a  $9\% \pm 4\%$  reduction of daytime fatal crashes, for an age-year-state group when not allowed to drink, compared with an age group when allowed to drink. For male drivers, the effect on nighttime fatal crashes was  $10\% \pm 6\%$ , and for female drivers  $26\% \pm 11\%$ . There seemed to be no significant effect for drivers 20 years old, but the difference against 18 and 19 year old drivers was significant. The cohort analysis found that a one-year increase in the drinking age resulted in a  $5\% \pm 4\%$  decrease of the combined 5-year accident experience of the cohort; this would correspond to a 20% reduction for an age-year-state group. Though this effect is larger than that found in the first analysis (13% for one year), the difference is not statistically significant.

In 1987, the United States General Accounting Office (GAO) reviewed and synthesized some 50 pertinent studies (U.S. General Accounting Office, 1987). It found that raising the LMDA generally reduces alcohol-related traffic crashes for the affected age groups. The amount of reduction attributed to the LMDA varied. For example, in four "sound" studies using data from several states, the reduction ranged from 5 to 28 percent. The GAO study also found that the available evidence

supported the claim that raising the LMDA also reduces alcohol consumption and driving after drinking. The GAO researchers found some evidence also of a small spillover effect for younger drivers who were not the direct target of the LMDA legislation. They concluded that there was insufficient evidence to assess the border crossing effect (the effect of different LMDAs in adjacent jurisdictions), or to assess the long-term effect of LMDA.

Several subsequent studies have also supported these positive findings. Among these is a study by Womble (1989) which examined the ratio of after-law to before-law fatal crash involvements per licensed driver for *potentially affected* drivers in 13 states during the period 1975-1986. For each state, the driver age groups affected by the change were combined for the years before the change, and the years after the change. The other ages in the bracket 18-23 years are similarly combined into a control group of *potentially unaffected* drivers. For each treatment group and each control group, the ratio of fatal accident involvements to licensed drivers is calculated and the ratios of these rates to those of the control groups compared "before" and "after." From this analysis, she estimated a 12% reduction ( $\pm 6\%$ ) in fatal crash involvements for the target age group.

Hoxie and Skinner (1987) studied the effect of raising the legal drinking age on the fatal crash involvement of 18-20 year old drivers, applying econometric techniques to FARS data for the 50 states and the District of Columbia during the period 1975-1984. The fatality event used to define the dependent variable was an accident involving a driver 18-20 years old. The dependent variable was the number of all persons killed in such accidents, divided by the number of persons 18-20 years old (the number of persons rather than the number of licensed drivers was used because the latter was not completely available). For the analysis, the logarithm of this rate was used, which allowed simpler interpretation of the results. Different independent variables were used in several models. All models used one variable which described which proportion of the 18-20 year olds were allowed to drink, considering the legal age limit, the time of change of the limit, and the effects of a "grandfather clause," if any. Data from 51 states over 10 years gave 510 data points for the analysis. A pooled, cross-section time series model was used. Four alternative models were studied. The authors found that the affected age group had between 9% and 13% fewer fatal crash involvements after the drinking age was raised than they would have had otherwise. The study's use of alternative analyses make this result appear very robust.

Hoxie and Skinner also examined two potential "spillover" effects in their 1987 study, using fatality involvement as a dependent variable. For the 14-17 year old group, no apparent effect was found. For the 21-23 year old group, a more subtle effect was hypothesized: that after raising the drinking age to 21, the drivers would be "inexperienced" in drinking and have a higher accident involvement than they would have, had they been "experienced" in drinking. To study this effect, the intervention variable was lagged by the number of years the drinking age was raised, and used to replace it in one of the models. Again, no spillover effect was found.

Skinner and Hoxie (1989) updated their 1987 study in a very careful analysis of LMDA laws. Their measure of effectiveness was percentage change in per capita fatalities involving 18 to 20 year old drivers. Their analysis used a pooled, cross-sectional econometric model controlling for such factors as economic effects and seatbelt adoption among the fatal population generally. They used a control group consisting of drivers aged 21 and over and defined a LMDA change as any change affecting anyone in the 18 to 20 age group to purchase *beer*. They estimated a reduction in fatalities of 10.3% to 12.8% attributable to the LMDA laws over the 1983-1987 time period studied. Differences in the effectiveness for the five-year period were not statistically significant, so no time trends in law effect could be estimated. The study found no spillover effect on 14 to 17 year olds (mainly because of relatively small sample size) or on 21 to 23 year olds.

Other, less extensive and / or less carefully-designed studies of raising the LMDA have also found a generally positive effect. Hoskin, Yalung-Mathews, and Carraro (1986) studied the effect of raising the LMDA in 10 states, in some from 18 to 19, in others 18 to 20, 18 to 21 or 19 to 21 during the period 1977-1980. The study populations were the affected age groups; in addition, drivers 25-29 years old were used as control groups. The time frame was the years 1975 through 1982. Nine out of 10 of the treatment groups showed a decrease, only one an increase, and a statistical test shows a significant reduction. On the other hand, only 3 out of the 10 control groups showed a reduction, and seven showed an increase; overall, there was no significant change. The ratios for treatment to control groups also showed a significant decline. However, the authors did not consider the possibility that the younger drivers might have had a declining trend fatality rate everywhere, not only in the treatment states, even if the considerably older control group did not show such a trend.

Lacey *et al.* (1989) assessed the traffic safety impact of the implementation of laws raising the LMDA from 18 to 21 (for beer, and 19 to 21 for other beverages) in South Carolina in 1984. Five age groups of youth (<18, 18, 19, 20, 21-25) involved in different types of traffic accidents were the target populations for their analysis. Three types of accidents were studied, 1) accidents in which the driver was judged as having been drinking by the investigating officer, 2) nighttime (8pm-4am) crashes, and 3) nighttime single-vehicle crashes. Five accident types were analyzed for each of the five age groups mentioned above, 1) all alcohol-related crashes, 2) serious and fatal injury alcohol-related crashes, 3) nighttime crashes, 4) serious nighttime crashes, and 5) single-vehicle nighttime crashes. The data were expressed as percents of total accidents (or total serious injury accidents). The time series window was from January 1982 to December 1987. The crash patterns for each of the directly affected age groups were examined with respect to several measures and tested for an effect due to change in minimum drinking age using June 1984 as the intervention point for 18 year olds, January 1985 for 19 year olds and September and October 1986 for 20 year olds. The study concluded that the laws have "resulted in reductions in alcohol-related crashes as measured by police report and proxy measures. However, the reductions observed are not as consistent and dramatic as might have been expected based on the experience in other states."

McKinnon and Woodward (1986) studied the traffic safety impact of raising the LMDA in Illinois (19 to 21 in 1980), Massachusetts (18 to 20 in 1979) and Michigan (19 to 21 in 1978). Missouri, Michigan and Connecticut were selected as control states. No reason for the selection was given; geographical proximity appears likely. The study compared times series of monthly numbers of driver fatalities 21 and younger, and 25 or older for the years 1975-1981; for the control states, only the younger group was used. This design had the disadvantage of not accounting for changing numbers of drivers in the two age groups. Despite this and other problems with the design and the statistical methods used, the results of the study suggest that there were positive effects in Illinois and Michigan, and a possible positive effect in Massachusetts.

Finally, Brown and Maghsoodloo (1981) studied the effect of *lowering* the LMDA in Alabama from 21 to 19 in 1979. The study involved a before-and-after analysis of single-vehicle accidents in Alabama. "Before" was defined as 1972-1974, and "after" was 1976-1979. The accidents were divided into two groups, an alcohol-related group and a non-alcohol related group. Single-vehicle accidents were classified as alcohol-related if so indicated in the police accident report or if the driver's BAC was positive. The percentage of 18-20 drivers (the authors included 18 year old drivers because they expected that lowering the drinking age to 19 would also affect 18 year olds) in the alcohol-related group was compared with the percentage in the non-alcohol related group, before and after the reduction in drinking age. On the basis of the statistical tests (mainly chi-square tests), Brown and Maghsoodloo concluded that a statistically significant *increase* in alcohol-related single-vehicle accidents occurred after the drinking age was lowered from 21 to 19 in Alabama. Other hypotheses for this increase were not explored in any depth. However, in a commentary on this study Koch noted in an article for *Accident Analysis and Prevention* that, after 1975, the total number of single-vehicle fatal accidents for the affected age group increased essentially the same as it did for other age groups (See full review in Volume II of this report). Koch speculated that possible explanations for the increase in the proportion of alcohol-related accidents for the 18-20 group could be (1) an increase in drinking but without increasing the risk of a fatal single-vehicle accident so that certain accidents which previously involved no alcohol were now involving alcohol, or (2) a change in alcohol reporting by investigating officers.

In an interesting study related to the LMDA, Wagenaar (1982) assessed the effects on aggregate beer and wine consumption of lowering and then raising the legal minimum drinking age in Michigan. In January 1972, Michigan lowered the drinking age from 21 to 18 years. In December 1978, it was raised from 18 to 21 years. Also, in December 1978, a 5¢ deposit per bottle or can of beer was introduced. Wagenaar analyzed time series of beer and wine distributions to wholesalers from breweries and wineries in the 1969-1980 time period. He found that wine distribution did not change significantly in 1972 after the LMDA was lowered, or in 1979 after the LMDA was raised and a mandatory container deposit law was implemented. Wagenaar also found that total beer distribution and package beer distribution did not change significantly in 1972, but that a significant decrease began in 1979. He concluded that part of the decline in package beer sales was offset by an increase in

draft beer sales. Wagenaar notes the inconsistency of the conflicting findings on beer and wine in supporting the "availability hypothesis" and provided some plausible explanations for them (which he then dismissed as "informed speculation.")

A careful look at Wagenaar's graph of 12-month moving averages of draft beer distribution shows that it was essentially constant through 1973, increased in 1972, and decreased somewhat early in 1973. However, from the middle of 1973 through 1977 it was essentially constant, remaining at a higher level than before 1972. Therefore, it appears likely that the higher level of draft beer distribution is the more important feature than the slight decline in early 1973. It is not implausible that lowering the drinking age should have had primarily an effect on draft beer distribution. As Wagenaar notes, draft beer is the least expensive alcoholic beverage, and might therefore be more attractive to younger persons than packaged beer. Also, draft beer is usually consumed on the premises where the seller can exert some control on who consumes the beer, whereas there is practically no control on the consumption of packaged beer once it is sold. Thus, the author appears overly cautious in dismissing his explanations of his conflicting findings in supporting the availability hypothesis.

Further, the 1979 intervention variable shows a significant decrease of packaged beer (-11.5%), and of total beer (-7.3%), but a significant increase in draft beer (+19.8%), and a very small, nonsignificant increase in wine. Wagenaar notes that the deposit law increased the real price of packaged beer by about 10%. He is careful not to interpret too strongly the decline in overall beer consumption as a result of limiting availability of beer for younger people, and of shifting from packaged to draft beer for older people. A closer look at his graphs for total beer distribution and for packaged beer distribution shows that the decline did not begin in January 1979, but might have started as early as late in 1977, early 1978. Wagenaar mentions an economic recession in Michigan, but does not give its dates, and does not attempt to quantify its impact.

In sum, Wagenaar's data seem to provide stronger support than he thinks for the hypothesis that lowering the drinking age had an effect on draft beer consumption. The effects of raising the drinking age cannot be separated from those of the deposit law. However, our interpretation of his data is that the deposit law had the greater effect. Nevertheless, the magnitude of the effect remains open, since a decline due to other factors might have started earlier.

## RESTRICTING ALCOHOL SALES

Several studies have examined the effects of various attempts at controlling alcohol availability by restricting sales at such retail outlets as bars, taverns and restaurants. Such an approach has appeared promising because many impaired driving trips originate at such drinking establishments.

Blose and Holder (1987) examined motor-vehicle crash patterns in North Carolina counties adopting liquor-by-the-drink (LBD). Prior to adopting LBD, properly licensed establishments could sell beer and wine to patrons, but patrons desiring to consume distilled spirits would have to provide their own spirits and purchase "set-ups" (ice and mixers) from the establishment.

Blose and Holder used a very elaborate research design in which two groups of counties were studied. One group issued permits in November 1978 and the other began issuing permits beginning in January 1979 until March 1979. Other counties were not included because they issued permits at different times, or issued only a few permits. For each of the included counties, a matched control county which did not permit liquor by the drink was selected on the basis of population changes and per capita income changes. Two dependent variables were used: accidents where the police indicated that a driver had been drinking (HBD), and single-vehicle nighttime accidents (SVN) involving males drivers age 21 or older. As a control variable, the study used single-vehicle nighttime accidents involving drivers under 21 years who should not have been affected by the change in law.

Blose and Holder fitted Box-Jenkins ARIMA models to the logarithms of the data for the period 1973 through 1982. The models for the two groups of treatment counties showed significant intervention effects for both dependent variables, and none for the control variables. As a second step, the times of assumed intervention effects were exchanged: January 1979 for the first group of counties; and November 1978 for the first group of counties. Then the coefficients of the intervention effects became either non-significant, or the fit of the models much worse. This supports the conclusion that the intervention effects appear at the expected times.

The control groups of accidents (drivers under 21 years) showed no intervention effect. The control counties for treatment group 2 did show a significant change in HBD accidents, but not in SVN accidents. None of the control counties showed a significant intervention effect for accidents of drivers under 21. The two treatment counties which allowed liquor by the drink showed increases of HBD accidents by 17% and 24%, the comparison counties 9% and 15%. SVN accidents showed increases of 14% and 16% in the counties allowing liquor by the drink, and practically no increases in the comparison counties.

Colon and Cutter (1983) examined the effect of several alcohol availability variables and beer consumption on fatal motor vehicle accident rates in the 50 states and the District of Columbia. The database used was FARS, and their approach and analysis incorporated a regression analysis. They found a significant correlation

between fatal accidents and beer consumption, suggesting that policies for controlling beer consumption might have highway safety benefits. However, several factors tend to weaken this finding, including the authors' use of percentage of metropolitan residents as a measure of the relative incidence of urban/rural driving and the percentage of drivers under age 21 as a measure of the effect of young drivers. Their statistical approach was also troublesome, especially their exclusion of several measures of alcohol consumption in their final analysis, including the alcohol equivalent of all beverages, and distilled liquor and wine, because they failed to relate significantly to motor vehicle fatal accidents and fatalities in their preliminary analysis. Their finding that average per capita beer consumption and the number of outlets are strongly correlated was based on measures that had population aged 16 or older in the denominator which tends to inflate existing correlations, and may even create spurious correlations.

Other measures for restricting alcohol sales have not been evaluated for their effect on traffic safety. However, a number of studies have examined the effect of such measures on various intermediate variables that may be related to traffic safety. Three such studies are reported here as examples of this type of research, although, strictly speaking, they fall outside of the scope of this project. Other reviews of other studies of this type are contained in Volume II.

One of these three studies examined the effect of banning so-called happy hours which involve proprietors attempting to attract patrons by selling drinks at reduced prices during specified time periods. Many jurisdictions in the United States have laws prohibiting happy hours for precisely this reason. Smart and Adalf (1986) examined the traffic safety effect of a such a law in Ontario, Canada. The purpose of the study was to examine how the ban affected 1) drinking in a bar with happy hours and 2) overall sales of alcoholic beverages in Toronto and 3) the number of charges of impaired driving. Data on drinking were collected by observations at two tables in each of 5 bars, two days prior to the banning of happy hours, and four weeks later. Monthly alcohol sales were obtained for the period October 1984 through February 1985, and the same period one year prior. Daily charges for impaired driving were obtained for the period November 1984 through January 1985, and the corresponding period one year earlier. The study showed no evidence of a decline in alcohol consumption subsequent to the policy change on both the individual and aggregate level. The authors present the proper caution and interpretations of the data, namely that the datasets were small, the reductions in impaired driving charges could also be confounded by the Christmas spot-checks and drinking driving campaigns, and by weather conditions.

The second of these studies was also by Smart and Adlaf (1987) who examined the effect of "age of majority cards" on drug and alcohol use among a sample of 3,600 students enrolled in grades 7, 9, 11, and 13 in Ontario (Canada) Public and Separate school systems. The data were drawn from a province-wide survey conducted in 1981. The study concluded that, "The most prominent finding is that, in comparison to those with no card and those who possess one, those who are compelled to use the card illegally represent a relatively deviant group. They are significantly more likely

to: 1) drink more frequently; 2) report more alcohol problems; 3) drink at a friend's home; 4) drink in pubs or taverns; 5) drink in cars and 6) drink at school activities. It appears that age of majority cards are a poor device for controlling the drinking of young people. They may, in fact, enable some underage drinkers better access to alcohol than they have without them" (p. 63). The last sentence seems to be an overstatement as we do not know if the drinking habit would be worse without them. Also the study is correlational so the cards in fact may have had nothing to do with "enabling" drinking.

The third study (Russ, Geller, and Leland, 1989) reviewed the results of several international studies that assessed the practice of some alcohol-serving establishments of placing devices on their premises that enable patrons to receive immediate feedback on their blood/alcohol level. Each of the eight studies summarized had its own research design. Generally, the studies report on the subsequent driving decisions of individuals who were provided feedback on their BACs. Also, generally, the subjects were categorized with respect to moderate drinking versus excessive drinking. The studies from the U.S., Canada, and New Zealand find, in general, that BAC feedback did not decrease the incidence of moderate or excessive drinking nor did it decrease the proportion of the intoxicated individuals who drove from the drinking situation. Moreover, some studies suggested that this BAC feedback may actually increase alcohol consumption, thereby undermining its potential for DUI prevention. The authors concluded that a more effective DUI countermeasure would be to educate and train the servers such as delaying drink service, offering food, serving non-alcoholic beverages, suggesting that a patron not drive, etc.

In summary, we found only two studies that evaluated the highway safety impact of restricting the sales of alcoholic beverages. The first, a carefully designed study by Blose and Holder, found that counties not permitting the sale of liquor by the drink in North Carolina had fewer alcohol-related crashes than did counties that permitted the sale of liquor by the drink. The second study by Colon and Cutter found a significant correlation between beer consumption and fatal crashes, but their findings were weakened by their analytic approach.

## INCREASING TAXES ON ALCOHOLIC BEVERAGES

It has been hypothesized that raising the price of alcoholic beverages, principally through increased taxes, will reduce alcohol consumption and therefore the number of alcohol-related crashes. Although several studies have investigated this approach, many of them have serious methodologic and analytic flaws. Nevertheless, two studies of the effects of increased taxes on alcoholic beverages produced some interesting results that are worth noting here.

The first of these (Walsh, 1987) was conducted in three parts. First, the author studied the relation between alcohol taxes and prices for beer and spirits in Ireland over the years 1950-1984. Regression models were derived which describe the relation between prices and taxes in real terms. Next, Walsh reviewed price elasticities for alcoholic beverages in Ireland, as published in the literature. Finally,

he studied the traffic death rate per 1,000 registered vehicles. Walsh found that prices increased with taxes and that consumption decreased with increasing prices. Further, Walsh found that traffic deaths per 1,000 registered vehicles increased with increasing consumption, leading him to conclude that increasing taxes on alcohol would decrease alcohol-crash risk. Some caution is required in interpreting this finding, since, as the author notes, "there is always the possibility that the influence attributed to alcohol consumption is really due to some other factor with which alcohol consumption is highly correlated over time." He mentions that replacing alcohol consumption by total personal consumption expenditures, gives only marginally worse results; also the unemployment rate would give almost as good a fit as alcohol consumption. Since it is well known that traffic deaths drop in an economic recession when personal incomes and expenditures drop, and unemployment increases, it is not possible to distinguish the effect of these factors from that of alcohol taxes.

The second study (Phelps, 1985) uses data from various sources to estimate the potential effects of changes in alcohol taxes on fatal accidents involving drivers 16-21 years old. The figures which he uses must be viewed with some skepticism, for example, that a 10% increase in the price of alcohol would reduce the frequency of daily drinking by 33%, the frequency of drinking six or more drinks per day by 30%, and the frequency of drinking three to five drinks per day by 16%. Most of these estimates are not significant, or are barely significant. In general, one should not expect that a rational drinker would reduce his drinking by a larger percentage than the price increase.

Both of these studies must be viewed as preliminary and speculative in nature, suggesting only that this direction of research should be pursued, but not offering any conclusive support for large-scale changes in public policy with regard to taxes on alcohol.

## SUMMARY AND CONCLUSIONS

The various studies reviewed in this section are summarized in the tables below. Our own comments on study findings are enclosed in brackets. The evaluations of countermeasures that raised the legal minimum drinking age include many that are among the best in the highway safety field. These evaluations clearly show that controlling the availability of alcohol by raising the minimum legal drinking age can markedly reduce alcohol-related fatalities.

Other attempts at controlling the availability of alcohol have rarely been evaluated to determine their traffic safety effect. Such countermeasures include restricting the sales of alcohol (for example, not permitting sales of liquor by the drink, banning happy hours, and self-testers for determining blood alcohol concentration) and placing increased taxes on alcoholic beverages. The one careful impact evaluation of countermeasures in the former group found that counties not permitting the sales of liquor by the drink had fewer alcohol-related crashes than did counties that permitted the sale of liquor by the drink. The evaluations of the latter group were plagued by

methodologic difficulties, but did suggest that raising the cost of beverage alcohol may merited further study.

Summary of Evaluations of Countermeasures Restricting the Availability of Alcohol<sup>a</sup>

Reference	Description	Design	Findings
Blose and Holder, 1987	A law permitting sales of liquor by the drink in several North Carolina counties.	1,3,4	The program did not increase alcohol-related accidents, suggesting a counter-program would not decrease accidents.
Brown and Maghsoodloo, 1981.	Lowering the LMDA in Alabama.	2	Increase in alcohol-related single-vehicle crashes after LMDA lowered (19 to 21).
Colon and Cutter, 1983	Effect of beer consumption and other availability variables on fatal accidents in the 50 states and DC.	2	This cross-sectional analysis found a significant correlation between fatal accidents and beer consumption. [Finding weakened by the research design used.]
DuMouchel, Williams, and Zador, 1986	Raising the LMDA in 26 states.	2,3,4	Nighttime fatal crashes reduced by 13% overall, 10% for males and 26% for females.
Hoskin, Yalung-Mathews, and Carraro, 1986	Raising the LMDA in 10 states.	1,4	90% of affected age group had crash reductions, compared to 30% of unaffected age group.
Hoxie and Skinner, 1987	Analyzed data from 51 states over a 10-year period to find effect of raising the LMDA.	1,3,4	A 9%-13% reduction in fatal crash involvements for affected age group. No spillover effect on other age groups.
Hoxie and Skinner, 1989	Update of their 1987 study using more controls.	1,3,4	A 10%-13% reduction in fatalities and no spillover effect on other age groups.
Lacey, <i>et al.</i> , 1989	Raising the LMDA in North Carolina.	1,4	Reductions in alcohol-related crashes for the affected age group.
McKinnon and Woodward, 1986	Raising the LMDA in three states.	1,3	Positive effects in at least two of the three states studied.
Phelps, 1985	Examined the relationship between alcohol taxes and fatal traffic accidents.	NA	Found that an increase in the price of alcohol would decrease alcohol consumption. [The numerical amounts of the price elasticities are not plausible.]
Russ, Geller, and Leland, 1989	A review of evaluations of breathtesters placed in bars to provide impairment feedback to customers.	NA	Studies in the U.S., Canada, and New Zealand found that the devices were generally ineffective and may even have increased alcohol consumption in some instances.

Reference	Description	Design	Findings
Smart and Adlaf, 1986	A law banning happy hours in Ontario, Canada.	2	No effect found. [There were many confounding factors that were not accounted for.]
Smart and Adlaf, 1987 <sup>b</sup>	Age of majority cards issued young persons in Ontario, Canada.	NA	The cards were found to be ineffective and possibly counterproductive.
U.S. General Accounting Office, 1987	Synthesis of prior studies on raising the LMDA.	NA	Raising the LMDA generally reduces alcohol-related crashes for affected age groups. Effect was 5%-28% in four sound studies.
Wagenaar, 1982	Effect of lowering and then raising the LMDA on beer and wine consumption in Michigan.	1,4	Decrease in total beer consumption of 7%. Concurrent new bottle deposit law may have contributed to effect of LMDA.
Wagenaar, 1983	Raising the LMDA in Michigan and Maine.	1,3,4	20% of all alcohol-related young-driver crashes can be prevented by removing access to alcoholic beverages.
Walsh, 1987	Studied the effect of various variables related to alcohol consumption on traffic deaths per vehicle in Ireland.	2	Found that traffic deaths decreased with decreasing consumption. [Result is not conclusive because of approach used (regression) did not adequately account for possible "other factors."]
Williams <i>et al.</i> , 1983	Raising the LMDA in nine states.	1,3,4	For target groups of the laws, fatal crashes reduced by 14%-23%, depending on type of fatal crash.
Womble, 1989	Raising the LMDA in 13 states.	2,4	Estimated a 12% reduction in fatal crash involvements per licensed driver for potentially affected drivers.

a. Codes for research design of general deterrence component are: 1 - time series 2 - other, non-time series; 3 - control jurisdictions; 4 - other controls.

b. Not an impact evaluation. Data based on survey responses.

## CHAPTER 3 - DETERRENCE AND INCAPACITATION

This class of countermeasures is based on the concept of deterrence which states that individuals can be deterred from engaging in a legally-proscribed behavior if they perceive that the risk of punishment for that behavior is sufficiently high. The job of creating such a perceived risk is performed for the most part by the agencies of the Traffic Law System (TLS) which pass laws, enforce the laws, adjudicate to determine the guilt or innocence of drivers accused of violating those laws, and impose sanctions upon drivers found guilty of a violation. Two forms of deterrence are sought in this approach, *general deterrence*, through which the TLS attempts to prevent a behavior among persons who have not yet been caught and punished, and *specific deterrence*, through which the systems seeks to prevent a behavior from re-occurring among persons who have been caught and punished. The TLS is also used to *incapacitate* law violators, either by physically incarcerating them in a jail or prison or by removing them from the legally driving population by taking away their driving license.

Specific countermeasures in this class usually have focussed on actions taken by the agencies that perform the primary functions of the TLS, that is, enforcement, adjudication, and sanctioning. In a few instances, jurisdictions have operated a comprehensive countermeasure program involving several TLS agencies, sometimes even in conjunction with other countermeasures such as treatment and rehabilitation (discussed in Chapter 4). The discussion in this chapter is organized by the TLS function most involved in a countermeasure. Some countermeasures that have employed a comprehensive approach involving more than one TLS function are also discussed.

### ENFORCEMENT

Voas and Hause (1987) examined the deterrent effect of increased DWI patrol activity in different sections of Stockton, California, during a 3½ year period starting in January, 1976. The experiment involved adding 10 extra patrol cars to the normal traffic law enforcement activity on Friday and Saturday nights between 8 p.m. and 4 a.m. Enforcement procedures were traditional, not involving sobriety checkpoints (see below) or preliminary breath testers. In 1976, arrests for DWI during the enforcement period increased by a factor of seven over the 1975 baseline year. The high arrest rates were maintained throughout the study period during the times of the year when the patrols were operating.

The authors analyzed accident time series for the period 1973-1981. The time series were reported accidents of all levels of severity occurring during (1) Friday and Saturday night, (2) weeknights, and (3) daytime. The accident analysis used ARIMA

methods following the approach of Box and Taio. The analysis considered Stockton alone and also compared the Stockton data with the aggregate of control city data over a 1973-1980 time period. The control cities were four cities in California, Fresno, Bakersfield, Modesto, and Riverside. The authors also analyzed the results of four roadside surveys in which driver BACs were measured.

The analysis of the accident data showed statistically significant decreases in the number of nighttime accidents occurring in Stockton. The authors found that this reduction amounted to 10% to 15% during the 3½ year period of the project and that there was no similar reduction in daytime accident accidents in Stockton or in nighttime accidents in comparison cities. Further, the study found that the number of drinking drivers on the roads during weekend evenings decreased during the project period - during the baseline period, 8% of the drivers stopped had a BAC of .10% or more, compared to 5% stopped during the project. The program's effect was found to be greatest during the early part of the program when there was more publicity about the projects, and an effect was also noted after the publicity died out.

Lynn (1985) reports the results of an evaluation of 13 local selective enforcement programs in Virginia. Each is evaluated in terms of its stated goals. An overall evaluation is also provided. In general, the programs involve setting up special enforcement patrols for DUI detection which then patrol the locality during specific target hours, Friday and Saturday nights. There is no mention in the evaluation report of any concurrent PI&E program. Each locality had stated goals in terms of increased DWI arrests and reductions in alcohol-related accidents in the project year as compared to the three previous years. Ten out of the thirteen localities met their stated goals in terms of reductions in alcohol-related accidents. For seven of these ten, the change was attributed to the program since alcohol-related accidents either did not decrease or decreased to a lesser extent during non-target hours.

Several studies have evaluated the effect enforcement programs incorporating a public information and education (PI&E) component. Lacey *et al.* (1986) evaluated the effect of an enforcement program in Clearwater and Largo, Florida, that was supported by a PI&E program that emphasized the specific enforcement strategies that were used. These strategies were quite diversified, ranging from sobriety checkpoints to saturation patrols to training in DWI detection to increased command emphasis in the two police departments.<sup>2</sup>

The effects of the campaign were evaluated by comparing the study jurisdictions, Clearwater and Largo, with comparison jurisdictions, Sarasota and Bradenton. To evaluate the impact of the traffic safety impact of the program two measures were used:

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<sup>2</sup> Though checkpoints were but one of several enforcement approaches implemented and publicized, they were by far the most frequently recalled strategy when licensed drivers responded to a telephone survey.

- crashes which were alcohol-related in the opinion of the investigating officer, and
- nighttime crashes.

The evaluation found that the intervention effect was significant, 12.5 and 20.3 percentage point drop, respectively, from a before-intervention level of 65%-70%.

Another evaluation of a similar enforcement / PI&E project in Indianapolis, Indiana, conducted as a part of the same project found only a possible effect, attributed in part to the inappropriate use of Cincinnati, Ohio, as a control site (Lacey *et al.*, 1988). Also, the Indianapolis campaign was less intense than the Clearwater-Largo campaign.

Pigman and Agent (1986) evaluated the Traffic-alcohol Program (TAP) in Lexington-Fayette County. The program included 1) officer DWI training course, 2) deployment of officers for DWI enforcement, 3) public information campaign, and 4) development and administration of an alcohol education program. About two years into the project, the following additional steps were undertaken: 1) creation of an accident reconstruction team, 2) measurement of BAC in all fatal accidents, and 3) development of a working partnership between the division of police and the Fayette County Commonwealth Attorney.

The program began in May, 1982. TAP enforcement was conducted from 10:30 or 11:00 PM to 3:00 or 3:30 AM on various days of the week, varying over the years. DWI arrests more than quadrupled during each of the first two years, compared with the year before; this declined to double during the fourth year. Also, fines increased and time between arrest and adjudication was reduced.

The authors find a 30.3% reduction in alcohol-related accidents for the four-years of the program compared with the two years preceding it, 36.6% during the tap hours and 25% during the other hours. They find a significant reduction of all accidents by 9.5%, and mention that statewide alcohol-related accidents decreased by 16.2%, whereas all accidents increased by 4.9%. No details of the analysis are given. However, if one looks at alcohol related accidents as a percentage of all accidents, it appears possible that the observed reduction is just the effect of a time trend, with possibly a 4% reduction due to the program. This, and the lack of details on the analysis casts some doubt on the stated cause-and-effect relationship.

Calderwood and Woods (1983) evaluated the impact of breath alcohol testing mobile units (BAT mobiles) in Albuquerque, New Mexico. Two such units were operational in that city, the first beginning in April, 1979, and the second beginning in June, 1981. Their evaluation used an interrupted multiple-time series design analyzing Wednesday-Saturday, nighttime fatal-plus-injury, accidents over a period of 10 years (1972-1981). This measure was transformed to a rate per gallon of gasoline sold to account for possible changes in VMT that could confound the analysis. Control series included those consisting of equivalent daytime rates in Albuquerque, and of rates in Santa Fe and Farmington, and the combined rates of other statewide

urban areas of New Mexico. Gasoline sales were obtained from state tax records. The authors conclude that the program reduced alcohol-related accidents, but that there were insufficient data to estimate the amount of the reduction.

Finally, Wolfe and O'Day (1984) report the results of their evaluation of a four-year project which combined selective enforcement and education in Oakland County, Michigan. Special police patrols were deployed in 10 target areas selected on the basis of having large numbers of alcohol-related accidents. Project goals were established in terms of increased enforcement (e.g., alcohol arrests), increased public awareness of drunk driving problems and increased knowledge of laws and enforcement efforts, and decreased alcohol-related accidents. The stated goals in terms of increased enforcement and increased public awareness were met, but increases in alcohol arrests in Oakland County were still quite similar to increases occurring in surrounding counties.

Frequencies of alcohol-related accidents at all levels of severity decreased in Oakland county during the project period, but so did accidents that were not alcohol-related. Very similar trends were also seen in other large counties and statewide. Alcohol-related accidents, as a percent of total accidents, showed very little change and did not meet the stated goal. Thus, although alcohol-related accident frequencies decreased, particularly in the target areas, there was little evidence to suggest these changes were due to the program.

Similar evaluations of the use of police enforcement combined with PI&E have been conducted in other countries. One such study of particular interest evaluated Great Britain's 1983 "Christmas Crusade" against drunken drivers, during which the police of England and Wales administered an average of 1,350 breath tests daily (Ross, 1987b). Although not deliberately planned and organized, the campaign, fueled by press reports, was perceived by many observers to be a nationally organized deterrent effort. Ross concluded that this program combining enforcement and PI&E was effective in deterring drunk driving for the short period during which it was in effect. He found a reduction in all fatalities in December, 1983, of 22.7% compared to a trend of 1.6% ( $p < .05$ ). A non-significant residual effect of 11.0% was observed in January of 1984. Ross found a non-significant *increase* in weekend nighttime fatalities during the treatment period which was dismissed because of a small N (not specified). He speculates that emphasis on certainty rather than severity of punishment was probably responsible for the decrease in all fatalities.

Vingilis, Chung, and Adlaf (1981) evaluated a DWI enforcement / PI&E program in Etobicoke, Canada, a borough of Metropolitan Toronto. The program used the acronym R.I.D.E. (Reduce Impaired Driving in Etobicoke). The enforcement component used random spot-checks and roadside breathtesting. The spot-checks were conducted over a two-year period starting in October, 1979 and were intensive, being conducted seven days a week, one or two shifts per day. Note that these checks were not sobriety checkpoints (discussed below), but were random stops without the requirement for probable cause as would be the case in the U.S. During the period of the evaluation (September 1979 through April 1979), 180,185 stops were made, and

2,051 breath tests were given. Of these, 451 drivers (22%) failed the test, and 14% were charged with an alcohol-related offense.

The PI&E component used a highly visible, electrically lit R.I.D.E. sign on the roof of the police cars. The sign was about 1.5 feet high and as wide as the roof of the car. (A subsequent survey found that 30.6% of the respondents first learned about the program through "police activity.") Print and electronic media were used, along with a pamphlet that was distributed to stopped drivers. There was no paid advertising except for the pamphlet. Forty-one percent of the respondents to the survey reported media as the prime source of information about the program.

Highway safety impact was assessed by comparing alcohol-related accidents (based on police judgement) in Etobicoke with those in the four other police districts in Toronto. Also, there was an independent roadside survey that provided data for 1974 and part of 1979 for three Toronto police districts, including the study district. Because of the small sample sizes for the jurisdiction studied, fatal accident data could not be used. It was not clear why other surrogates (e.g., nighttime accidents or nighttime injury accidents) were not examined. Analytic methods used in the evaluation included t-tests and time series analyses.

The authors concluded that there was no measurable highway safety effect, either on accidents or drivers using the road. The study found through self-reported data that the program increased the perceived risk of the "average man being caught," but not of the respondent being caught. Awareness of the program and program messages was also higher in Etobicoke than in the other police districts.

There have been several evaluations of a particular enforcement *strategy* that has been used both in the U.S. and in several other countries, particularly Canada, Australia, and France. This strategy has been given a number of names, but the most descriptive is "sobriety checkpoint." The successful enforcement / PI&E program in Clearwater and Largo, Florida, described above made heavy use of this strategy. In the U.S., sobriety checkpoints are most often conducted at night and are well-publicized in advance, although their specific locations usually are not announced. Very often, a large team of police officers and their vehicles are involved. Typically, officers direct groups of stopped vehicles into an observation area and engage the drivers in a conversation during which the drivers are observed for signs of intoxication. Stops are made in such a way that each vehicle has about the same chance of being stopped (for example, every tenth car). Behavioral tests for alcohol-impairment may follow. In jurisdictions so equipped, the drivers may be asked for a sample of their breath for a chemical test of their BAC by a Preliminary Breath Testing device (PBT). If the tests indicate impairment, the driver may be arrested for drunk driving.

A variation of sobriety checkpoints is widely used in Australia. Called *random breath testing*, it involves stopping drivers as they pass a stationary point and requesting all of them to undergo a preliminary breath test. If the test indicates no

alcohol, they are free to go, but if the test is positive, they must submit to an evidentiary test (Jones and Lacey, 1989).

We found two *impact evaluations* of sobriety checkpoints in the U.S.<sup>3</sup> The first is reported by Voas, Rhodenizer, and Lynn (1985) who evaluated the Driver's License and Sobriety Checkpoint program operated by the Charlottesville, Virginia, Police Department during December 30, 1983, to December 31, 1984. A total of 94 checkpoints were conducted during this period, and 23,615 vehicles were stopped. 290 drivers were arrested for DWI and another 386 were given "safety advisories" when their BAC was below 0.10% and they did not show any visible signs of impairment. The authors analyzed time series of alcohol-related accidents and nighttime accidents in Charlottesville and compared them to all accidents in Charlottesville and alcohol-related accidents and nighttime accidents in the state as a whole. The series included 36 months of data prior to the intervention and 12 months of data during the intervention.

The data indicated a significant decrease in *alcohol-related* accidents in Charlottesville during the checkpoint operations. The decrease amounted to roughly 15%. However, alcohol-related accidents also declined in the remainder of the state, so that when the Charlottesville data were compared with data for the rest of the state, the decrease in Charlottesville was not statistically significant. The data also showed that while there was also a decline in *nighttime* accidents in Charlottesville, the decline was not significant.

Levy, Shea, and Asch (1989) (see also Levy, 1988) evaluated the traffic safety effects of sobriety checkpoints and other local DWI programs in New Jersey. The program included two components that were principally consciousness raising efforts, SOBER (Stay Off the Bottle and Enjoy the Road) and DWTF (Driving-While-Intoxicated Task Force), and a third component, Strike Force, which involved the implementation of sobriety checkpoints. The approach to the evaluation involved using covariance equations involving annual values for twenty-six counties over a six year period from 1980 to 1985 for a total sample of one hundred twenty six cells. Dummy variables were created for each county, each year and each type of intervention. Control variables were added for unspecified demographic, highway and geographic elements within a specific county, but there was apparently no control group, *per se*. In addition, a time dummy variable for each of the six years was entered into the covariance analysis. In a separate analysis, not reported, the authors

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<sup>3</sup> A study by Williams and Lund [Williams, AF; and Lund, AK. (1984). Deterrent effects of roadblocks on drinking and driving. *Traffic Safety Evaluation Research Review* 3(6):7-18] assessed the effect of roadblocks on public perceptions using telephone surveys. The responses of residents of Montgomery County, Maryland, a jurisdiction which was making extensive use of well-publicized roadblocks, were compared to those in Fairfax County, Virginia, another Washington, DC, suburb. The perceived risk of arrest was much higher in Montgomery County. A similar survey of citizens residing in the state of Delaware and in the Maryland eastern shore was also conducted, with similar results. However, there was no evidence of an effect on self-reported DWI behavior.

derived weights for the number of years that each of the three programs studied, SOBER, DWITF and the Strike Force, were in place.

The study concluded that across all of the counties in which the three programs were applied, the Strike Force had the strongest effect, producing a reduction in nighttime single-vehicle accidents of approximately 10 to 15%. The study also found that DWITF produced a reduction between 6 to 10%, but that reduction was effective over a shorter period of time. The purely public information program SOBER did not produce a significant effect.

There have been several evaluations of Australian variants of sobriety checkpoints. One of the first of these was reported by Cameron, Strang, and Vulcan (1981) who evaluated the use of short-term (two-week) intensified random breath testing (RBT) in various "non-overlapping" sectors of Melbourne, Australia. The effectiveness of RBT was measured in several ways.

*First*, the number of fatalities during the intensified RBT weeks (and two subsequent weeks) was compared with the number during the same period of the previous year. There was a substantial reduction, but this cannot be clearly attributed to the program, since historical differences between the years are not controlled by this design. This problem is ameliorated *somewhat* by the use of "control weeks," during which no RBT was conducted during either year, and during which there was no noteworthy difference in fatalities across time. *Second*, the number of serious casualty accidents (those that resulted in a fatality or a hospitalization) was compared between sectors of Melbourne exposed to intensified RBT and those not, or not yet, exposed. Some comparisons showed a reduction, while others did not. *Third*, hospital BAC records were matched with police accident reports to determine whether the proportion of driver casualties with BAC above .05% decreased. Again the results were somewhat contradictory, but generally appear to indicate that RBT did reduce the proportion of drivers killed who had a BAC greater than .05%. Extreme caution is necessary when considering this result, however, since *only 55% of drivers who were killed or taken to a hospital* could be matched with hospital BAC measurements.

McLean (1984) incorporated roadside surveys into the evaluation of random breath testing as it was adopted in South Australia. Random breath testing was a controversial issue there and generated considerable publicity while it was being considered by the legislature, and while awaiting actual implementation after passage by the legislature. The roadside surveys revealed a 14% reduction in the proportion of drivers at or above a BAC of .08% after the law went into effect, but that reduction had disappeared within a year. A similar reduction in positive BAC's below the legal limit was also observed, and that reduction was maintained. This led the researchers to hypothesize that social drinkers were more likely to be deterred by the random breath testing program as it was being implemented (with limited resources and with stop rates well below those in New South Wales). In addition, in South Australia, random breath testing was confined to main roads, and this evidently led to a displacement of high-BAC casualty crashes to secondary roads as high BAC drivers attempted to avoid detection. Frank (1986) reported a similar displacement

in Melbourne. A subsequent intensification and diversification of random breath testing in South Australia has resulted in more marked declines in high-BAC drivers tested in roadside surveys. McLean also examined the effect of random breath testing on crashes, but their results were inconclusive.

Finally, Ross, McCleary, and Epperlein (1982) reported their evaluation of the impact of the passage by the French parliament on July 12, 1978, of a law which provided for random testing of drivers using a preliminary breath test at the roadside. The authors use a number of dependent measures varying from all crash related injuries and crash related deaths to more refined alcohol related measures such as crash related deaths on weekend nights. The basic method utilized by the authors is time series analysis. Where sufficient data is available over a number of years, a time series with seasonal variations removed is used for analysis of the impact of the intervention. The procedures used permit the authors both to estimate the number of injuries or deaths saved by the intervention, as well as the period of time over which the intervention appeared to produce the reduction in injuries.

Ross and associates found that the passage of the random testing law in France produced a temporary reduction in crash related deaths and injuries. This reduction was apparently not accounted for by any reduction in the number of miles driven, but may partly be accounted for by (or have caused) a reduction in wine sales. One hypothesis for the disappearance over time of the apparent deterrent effect was the failure of the French police to take full advantage of the new law. Not many drivers were arrested at the checkpoints and, in time, the French public may have come to recognize this.

To summarize, a number of *enforcement* countermeasures have been evaluated for their highway safety impact. Several of these had strong or adequate designs. Of these, the following indicated a positive effect:

- Using public information and education in support of various enforcement strategies such as sobriety checkpoints and saturation patrols;
- Increasing patrol activity several-fold; and
- Using breath alcohol testing mobile units (BAT mobiles) to process suspected drunk drivers more efficiently and effectively.

In addition, the use of sobriety checkpoints has been evaluated in two settings. The results suggested a positive effect, but were not conclusive.

## ADJUDICATION

Only two evaluations of this type were found. The first dealt with the principle of *implied consent*, which states that when a person drives, that person implicitly consents to submit to a chemical test when asked by a police officer to do so if

probable cause leads the police officer to believe the driver is driving while drunk. This principle is based on the premise that implied consent laws enhance DWI prosecution by providing evidence that will increase the probability that a drunk driver who has been caught by the police will be found guilty of DWI by an adjudicative agency. Drivers who refuse a chemical test are punished by having their driver's license suspended or revoked, usually for a longer period than that which would result from a conviction for DWI.

Sadler (1986) studied the efficiency, effectiveness and impact of California's implied consent program using 1982 data. The law provides: a six-month suspension for drivers with no prior DWI alcohol-related reckless driving convictions; 12 months suspension for one prior; and three years for two or more priors. Sadler collected three samples from implied consent case files of test refusers. Sample 1 (n = 2,050) was a 10% random sample of those who did not request a hearing. Samples 2 and 3 were those who requested a hearing and either had the suspension upheld or had the suspension set aside, respectively. Sample 2 was 20% of the upheld group (n = 1,364), and sample 3 was all of those whose suspensions were set aside (n = 1,050). These samples were selected for the case processing part of the study. The traffic safety impact study used samples from the groups 2 and 3 (888 and 533, respectively), and used analysis of covariance techniques with repeated measurements (three at six-month intervals). Interaction effects (treatment  $\times$  time, treatment  $\times$  age, and treatment  $\times$  time  $\times$  age) were tested at the .10 level, and each hypothesis of no treatment effect was tested at the .05 level.

Sadler's results demonstrated that suspending refusers can be an effective countermeasure for the studied subgroup of the DUI population. During the six-month suspension period, refusers whose suspensions were reinstated after an administrative hearing had significantly fewer alcohol-related accidents (63.7%), nonalcohol-related accidents (76.5%), and total accidents (72.2%) than did refusers whose suspensions were set aside. Sadler also found that refusers were only slightly less likely to be convicted of their DWI charge than were non-refusers (61 % vs. 66%), suggesting that the basic rationale for having implied consent in the first place may not be valid.

Salzberg and Klingberg (1983) evaluated the effect of Washington's deferred prosecution law which allowed a person charged with a misdemeanor or gross misdemeanor to have that charge dismissed if that person successfully completed an approved treatment program. The authors compared various measures of recidivism for the deferred prosecution (DP) group with those for a control group that did not receive DP. The analysis was retrospective, so random assignment to the two groups could not be used. However, another technique, covariance analysis, was used to control for predeferral differences between groups. Covariates were age, sex, and driving performance (e.g., accidents and traffic violations). The study found no positive effect for the DP program, and if anything, a small negative effect.

## SANCTIONS

Sanctions provide the ultimate "clout" of the Traffic Law System and have been the subject of a wide range of evaluations. Types of sanctions with acceptable evaluations in the time frame addressed by this project include:

- drivers license,
- jail, and
- probation.

In addition, we reviewed a group of studies not fitting neatly into any of these categories (usually because they addressed the simultaneous use of multiple countermeasures).

### *Drivers License*

The 1980 study by Hagen, McConnell, and Williams is the seminal evaluation of this class of sanction. It reports the results of a study in California to determine the effect of driver license suspension on accidents, DWI, and driving during the period of suspension. The study found that first offenders without a suspension had a higher crash risk and DWI conviction risk than did second offenders with a suspension. Suspended drivers continued to drive, but drove less frequently.

The research design involved driver-record searches (accidents and DWI convictions) of first offenders, second offenders, and third offenders (N=4,488); and questionnaire surveys of 2,500 drivers who were having their license reinstated after having their license suspended or revoked for multiple DWIs. The authors conclude that license suspensions are effective in reducing recidivism during suspension (both accident and DWI), and that suspended drivers drive less than they would have had they not been suspended. The authors speculate that there may be a high-risk group of first offenders that could be identified through pre-sentence investigation for license action.

A second study by the same authors (Williams, Hagen, and McConnell, 1984) analyzed the recidivism of (1) DWI first offenders who received no driver license suspension; (2) second offenders who received a 12 month suspension; and (3) offenders who received a 36 month revocation in the index year of 1974. The data were all from the California driver records system. The total number of drivers studied was 4,488, including 1,769 first offenders, 1,808 second offenders, and 911 third offenders. The study compared the numbers of surviving drivers for each of four years after their index conviction, where "surviving" was defined as not having received a DWI conviction, not having been involved in a reported accident, and not having been cited for a failure to appear in court or a failure to pay. Proportional tests using a z statistic were conducted annually to compare the percentages of surviving drivers among the three target groups.

The authors found that first offenders not receiving any driver license action had the lowest DWI survival rate, followed by third offenders. The one-year DWI survival rate of the second offenders was significantly higher than that of the other two groups, but the rates of the second and third offenders began to catch up and then surpass those of the first offenders as time passed. However, even after three years, the rates of the first and second offenders were almost identical, and there was only a small difference between the rates of these two groups after four years. The accident survival rate of first offenders was even worse in comparison with that of second and third offenders and remained so even after four years. The authors concluded that the first offender represents a significant crash threat and suggested that driver license actions be taken for this group.

In 1981, Salzberg and Klingberg (1981) evaluated the effectiveness of revoking the driver license of a special class of drivers, habitual offenders, in the state of Washington. The Washington habitual offender act required a five-year license revocation for drivers who had accumulated three or more major traffic violations or 20 or more total violations. The law permitted a "stay" of revocation for alcoholic drivers who undertook an approved alcoholism treatment program. The major violations included DWI. Stayed drivers did not receive the revocation at all unless the driver subsequently was convicted of one of the major offenses. During the time period of this evaluation, the program was administered by the courts rather than administratively.

The subjects in the Salzberg and Klingberg evaluation were male habitual offenders (HO). The law was evaluated with respect to the effect of its two major provisions, revocation and stay of revocation, on subsequent driving record, including accidents and traffic violations. The subsequent driving records of drivers receiving these sanctions were compared with those of control groups of drivers. There were three such groups:

- 1) Subjects selected for HO prosecution who were apparently not prosecuted;
- 2) Subjects the court could not locate; and
- 3) Subjects who had their case dismissed.

Subsequent alcohol-related accidents were analyzed separately. The post conviction records of the experimental group were compared with those of the various control groups. The means of the accidents and violations were used in the comparisons. Of interest to this review are the findings with respect to DWI. There were no group differences, all groups averaging nine DWI convictions per hundred per year. A separate analysis examined an early-reinstatement subgroup (N=55) and found no significant differences with respect to driving performance in the revoked period and driving performance in the reinstated period. Also analyzed were the stayed drivers who subsequently had their license revoked because of another violation. During the stayed period, these subjects had 83 DWIs per 100 drivers per year, compared to 9 violations per 100 drivers per year during the revocation period that followed. The study concluded that license revocation is an effective counter-

measure, and that there was no evidence that either stay of revocation or treatment was effective.

More recently, Jones (1986) evaluated the effectiveness of revoking the driver license of habitual offenders in Oregon. Habitual offenders were defined as drivers who had accumulated three or more major traffic offenses within a five year period. The offenses were reckless driving, DUI, hit-and-run, driving while suspended or revoked, eluding, and violations which constitute major offenses, such as assault and manslaughter or murder with a motor vehicle. After a person accumulates two major traffic offenses within a five-year period, a warning letter was sent stating that another major offense will result in license revocation. The study compared the subsequent driving records of 594 revoked drivers with the records of 522 drivers who were sent a second warning letter rather than a notice of revocation. This latter group can be considered as a non-equivalent control group.

Pre-test and post-test comparisons between the revoked and control groups were made using t-tests. Pre-tests compared the groups with respect to age and sex composition, major prior convictions, non-major convictions, accidents, and all driving involvements. The revoked group had a significantly higher rate of major convictions per driver. The post-test compared driving records of the two groups with respect to driving while suspended/revoked, other major violations, high risk non-major violations, driving uninsured, other non-major violations, and accidents. The revoked group had significantly lower rates of both high risk and other non-major violations and of driving uninsured. Comparisons were also made with a third group consisting of revoked drivers who did not receive the revocation notice.

Jones concluded from the pattern of differences that revoked drivers (at least those who received notices) probably drove differently as well as less often as a result of revocation.

Helander (1986b) evaluated of California's habitual traffic offender law, analyzing the outcomes of arrests of a sample of 1849 drivers meeting the criteria for being an habitual traffic offender (HTO). The author found that less than 4% were prosecuted as a HTO and only 1% were convicted, even though the fatal/injury accident mean of the HBO was 10 times that of drivers in general. Helander also found the new law to be ineffective due to lack of prosecutor cooperation, primarily because of double jeopardy considerations.

Marsh (1987) evaluated another "habitual offender" program with driver-license sanctions in California. The program, called the Negligent Operator (Neg-Op) Program, was a four-level program for drivers who have accumulated points for driving-related violations, including DWI. The program evaluated operated at four levels. First, warning letters were sent to drivers who accumulate two points in one year. (DWI carries two points.) Second, a more severe letter stating an intent to suspend was sent after the driver had one point less than the *prima facie* definition of Neg-Op (four points in one year, six points in two years, or eight points in three years). Third, a probation hearing was ordered for a driver who had reached the

*prima facie* definition, has accumulated two major convictions, or had accumulated three alcohol-related entries (including failure to appear in court) in three years. Driver license sanctions for violation of the probation conditions were imposed at the fourth level of the program. Alcohol-related incidents were treated differently than other incidents, for example, different warning letters, different kinds of probation conditions in the hearing, etc.

Subjects were assigned randomly to a treatment and a control group. Extremely large sample sizes were used in the study: for example, the alcohol group at level 1 had a treatment cohort of 32,453 and a control cohort of 8,090. The analysis covered a two-year period and used a survival-analysis approach.

Marsh found that this administrative program was effective both in reducing accidents of all types and in reducing traffic violation convictions. For accidents, the control group had a probability of an accident on or before one year of .138, compared to .121 for the treatment group, a difference of 13%. The author concluded that the program had prevented 2,172 accidents and 31,330 citations at cost of \$889 per accident prevented. The author also found that the small observed effect of the warning letters on accidents was not statistically significant. Most important, Marsh found that the program's effect on the alcohol subgroup was also not statistically significant. An interesting side result was the finding that the accident risk of the alcohol group was substantially less than that of the non-alcohol group. In fact, the risk of the alcohol *control* group was significantly less than that of the non-alcohol *treatment* group. The author explained this (plausibly) by stating that the alcohol group had already received relatively severe court sanctions and by the fact that the two-point assessment for a DWI probably brought more alcohol-group drivers with fewer violations and accidents into the program than did the assignment criteria for the non-alcohol group.

The Helander and Marsh studies illustrate how the effectiveness of different approaches to achieving similar objectives can differ dramatically. The habitual traffic offender program evaluated by Helander failed because of perceived legal constraints to adjudicating cases in a judicial setting. The Negligent Operator Program evaluated by Marsh achieved some success, apparently because it was executed in an administrative setting not requiring adherence to the more stringent standards of a criminal proceeding.

The California State Department of Motor Vehicles (1986) conducted an analysis to determine whether warning letters and informational materials could successfully augment other DUI countermeasure efforts, resulting in reductions in subsequent accident and DUI recidivism rates. Two issues were addressed: frequency of mailing and type of warning letter. The study addressed all drivers who had been convicted of a first offense DWI (no priors for the past five years). The sample contained only drivers over the age of 21 with a valid California license.

The research design used treatment plus control groups with "quasi-random" assignment adjusted for covariates. The design was not completely random because

of the necessity to select different treatment groups at different times. However, the time period over which all the groups were selected was only 4 1/2 months. There were three treatment groups: NOTS Group which received the standard warning letter that had been used in the past; Experimental Group I which received one mailing of the experimental warning letter; and Experimental Group II that received two mailings (separated by seven to 12 weeks) of the experimental letter. For the frequency-of-mailing analysis, a  $2 \times 2$  factorial analysis of covariance as implemented in the SPSS MANOVA procedure was used. The two factors were frequency of contact (1 or 2) and selection time period (2 or 3). For the type-of-letter analysis, a  $3 \times 3$  incomplete-design factorial analysis of covariance (SPSS MANOVA procedure) was used. The factors were type of warning letter (NOTS, experimental, or control) and time period (1, 2, or 3).

Warning letters were found to provide no significant reduction in accidents or convictions for DUI offenders. Neither the frequency of mailing nor the type of warning letter appeared to make any difference. It was recommended that none of the letters studied be implemented.

Several studies have compared the effects of license sanctions with the effects of other sanctions and Traffic Law System actions. Kadell and Peck (1982) evaluated a California program implemented in 1978 to deal with drivers with two or more "major" traffic violations within a three-year period. Major violations defined as hit-and-run, reckless driving, driving under the influence, and vehicular manslaughter. The program allowed the Department of Motor Vehicles to re-examine such drivers and to impose driver-license sanctions. The authors found the re-examination to be effective in reducing subsequent traffic law convictions, and, possibly, in reducing injury accidents and total accidents.

The study used a true experimental design with random assignment to treatment and control groups, and measured the subsequent eight-month rate of various types of accidents for the treatment group and the control group. It also analyzed other measures of effectiveness, including conviction rates for various violations, including DWI.

The authors conclude that "a substantial and statistically significant reduction in traffic reconvictions was attributed to the re-examination," and that "there was no evidence that the effect of treatment was greater on alcohol-related accidents than on those not obviously alcohol-related."

Another California study assessed the effectiveness of an alcohol treatment program for multiple-offender DWIs given in lieu of driver license suspension (Sadler and Perrine, 1984). The treatment program lasted for 12 months and was multi-faceted. The license suspension was also for 12 months. The program was implemented in four counties. There were treatment and control groups with random assignment in each county. In addition, there were four carefully matched control counties that did not implement the program. The subjects were 7,820 drivers who received a second or subsequent DWI conviction between January, 1976, and

February, 1977. Of these, 2,534 were from the four program counties and received treatment; 2,420 were from the program counties and received the license suspensions instead of treatment, and 2,866 were from the matched counties and received license suspensions but no treatment. The analysis of these used analysis of covariance and z-test methods.

Sadler and Perrine concluded that the alcohol-treatment group had 70% more nonalcohol-related accidents than the license-action recipients. Drivers receiving three-year suspensions had fewer nonalcohol-related accidents and convictions than those who received one-year suspensions. However, the alcohol treatment group and the license suspension group had the same number of alcohol-related accidents. Further, the alcohol treatment group had 9% fewer alcohol-related convictions than the license suspension group. Nevertheless, the license-suspension group still fared better with respect to alcohol-related and nonalcohol-related accidents combined. Another evaluation of license suspension vs. treatment for second offenders showed that the suspended group had a subsequent crash risk close to that of the average driver, while the treatment group had a crash risk much higher than the average driver.

Tashima and Peck (1986) examined the driving records of convicted first and second offense DWIs who received various combinations of traditional and non-traditional sanctions in California. The bulk of the study dealt with two groups, a "suspended group" that received driver license suspensions, and a "restricted group" that received a restricted license plus treatment. The countermeasure program consisted of "an alcohol-related driver improvement course for most first offenders and an approved 12-month treatment program [for second offenders]." The report indicates that great care was exercised in selecting subjects. The attributes of the subjects were examined in considerable detail to help adjust for the lack of random assignment in the study.

The study used a quasi-experimental design. Random assignment to the various treatment groups was not possible, but attempts were made to adjust for any difference among the subjects in the various groups through analysis of covariance methods. Covariates included age, sex, prior driving-related variables, and "ZIP-code related variables" such as rate of injury accidents associated with each subject's ZIP code. There were two separate types of analyses, each using a different set of analysis techniques. Study A analyses compared the driving records of each of the treatment combination. The comparisons were made at two post-treatment time periods conducted at six-month intervals. Study B analyses compared the recidivism of those convicted prior to certain reforms in the DWI laws with the recidivism of those convicted after the law became effective. The comparisons made in Study A used analysis of covariance methods and also examined the possible effects of any regional differences in study outcomes. Study B used a technique employing a log-linear analysis with odds ratios.

The major conclusions of the Tashima and Peck study were summarized as follows:

"Among second offenders, the suspended group had significantly lower rates compared to the SB 38 [restricted] group on the 1-year post-treatment nonalcohol, fatal / injury, and total accident measures. On alcohol accidents, the rates between the two groups did not differ significantly. On subsequent major convictions (including DUI), the restricted SB 38 group had a significantly lower rate than that of the suspended group.

"In the first offender analysis, there was a significant overall difference among the groups with the following pattern of results. First offenders who were given stronger license control sanctions incurred accident and conviction rates that were lower than those of offenders given lesser penalties. The suspended group had the lowest total and nonalcohol (1-year) accident rate, while the restricted program group had the lowest 6-month alcohol related accident rate. The restricted plus program group and the restricted-only groups had the lowest rates for 1-year major convictions.

"Findings from the pre-post AB 541 analyses indicated that AB 541 had an impact in lowering alcohol accident, total accident and major conviction rates among DUI drivers."

The authors also observed that the evidence continues to show that license suspension is more effective than license restriction plus treatment for reducing the net accident risk of multiple offenders, but that the picture is less clear for first offenders. They note that the study suggests that first offenders are either more responsive to treatment or less responsive to suspension than are repeat offenders. However, first offenders in both the suspended and the restricted groups had more accidents and minor traffic violations than did multiple offenders. The explanation is offered that there is an "overlapping" of alcohol-related driving behaviors among first and repeat offenders. With respect to major violations, the second offenders had higher rates than did first offenders, but the percent reduction in major violations was greater among repeat offenders in the restricted group than among first offenders in the restricted group. It was hypothesized that this may be due to the more extensive treatment given to the repeat offenders.

Peck (1987) summarized and reviewed the studies by Kadell and Peck, and Sadler and Perrine, as well as other countermeasures of this general type. He concluded overall that license suspension is generally more effective than rehabilitation in reducing alcohol-crash risk among DWI offenders, and that using both sanctions together is better than using either alone. Peck also found that the tougher sanctions and the *per se* law introduced in California in 1982 reduced the incidence of alcohol-related crashes and DWI recidivism. The *per se* law made having a BAC of .10% or more an offense in itself not requiring further proof of alcohol impairment.

Blomberg, Preusser, and Ulmer (1987) examined the specific and general deterrence effects of Wisconsin's 1982 law mandating three to six months license suspensions for first-time convicted drinking drivers. The law went into effect in May, 1982. Other provisions of the law included: a .10% illegal *per se* feature; possible vehicle impoundment for driving while under suspension; a \$150 surcharge on the fine; and a provision discouraging plea bargaining. The effect of the law was studied both

statewide and within Milwaukee County where a PI&E campaign supported by this project was conducted. The analysis of accident data used time series of a surrogate of alcohol-related accidents: single-vehicle injury and fatal accidents involving male drivers which occurred between 10:00 pm and 4:59 am on Thursday, Friday, or Saturday nights. A comparison series excluding single-vehicle injury and fatal accidents occurring between 10:00 pm and 4:59 am from all Wisconsin accidents was used to control for reporting changes and trends. No comparisons were made with other states not having the intervention, but comparisons were made between Milwaukee County and Brown County to determine the effect of the PI&E campaign.

The analysis of the effect of the law on subsequent accidents and DWI convictions dealt with four groups of drivers. Group 1 had a DWI conviction during the period May 1, 1980 to April 30, 1981 (pre-law), but none during the previous one-year period. Group 2 had a DWI conviction during the period May 1, 1981 to April 30, 1982 (post-law), but none during the previous one-year period. Two comparison groups of drivers with moving violations in the pre-law and post-law periods, respectively, and no DWIs in the previous year and moving violations in their respective prior year were also studied. The analysis dealt with the number of accidents / convictions that had occurred one year after the index conviction.

Box-Jenkins techniques were used for analyzing the accident data. The recidivism analysis employed survival analysis methods. Data for the comparison groups of drivers described above were used to control for possible differences in enforcement / adjudication and crash likelihood in the pre-law and post-law periods. Blomberg and associates concluded that the change in the law was associated with a reduction in alcohol crashes (as defined in the study) of about 110 per month statewide, and about 10 per month in Milwaukee, a 25% reduction. The authors thought that the media campaign may have produced a reduction of another 15% in Milwaukee which occurred during the first four months of the campaign.

The study also found that the new law reduced DWI recidivism and subsequent accidents in Wisconsin. The weighted reduction in recidivism amounted to about 10% (6.1% to 5.4%), and the weighted reduction in injury and fatal crashes was about 20% (2.7% to 2.1%).

A follow-up study reported by Preusser, Blomberg, and Ulmer (1988) added additional years to the original study and found that the average number of alcohol-related crashes decreased about 25% after the new law. It also found that an intensive media campaign conducted by the Safety Commission in Milwaukee in early 1985 increased awareness of the law from 48% to 75% and increased the belief that everybody convicted of DWI actually loses their license from 10% to 30%. Recidivism analyses indicated that re-arrest rates decreased nearly 50% after the adoption of the new law in the first six months after conviction, and that this reduction was maintained throughout the reporting period. We note that both of the Wisconsin studies did not compare their accidents series with those in other states that had weaker license laws, a procedure that would have strengthened their conclusions.

In recent years, an increasing number of states have begun to use an administrative process involving the implied consent principle discussed above to impose driver license sanctions more swiftly than had previously been possible. This concept, called administrative *per se*, has been the subject of several evaluations. Studies by Ross (1987a) and Lacey *et al.* (1989) in separate states adopting such laws have found that the adoption and awareness of administrative license revocation was associated with general deterrence as measured by decreases in alcohol-related crashes and their proxies.

Ross examined trends on the proportion of drivers or pedestrians in New Mexico with a BAC of .05%. He observed a decline of approximately 10% coincident with implementation of the administrative license revocation law. These results were obtained despite a relatively low awareness of the particulars of administrative *per se* (37% of respondents were not aware of the provisions of the law). The reason for including pedestrians in the analysis is not clear.

The experience in Nevada indicates the importance of public awareness of the law to its effectiveness. Lacey, *et al.*, reported the proportion of crashes occurring at night were reduced by approximately 10% after implementation of the law and by a further 7% after the law was publicized. We note that other countermeasures were introduced in Nevada at the same time as administrative *per se* which could have contributed to the reported effect.

Vingilis *et al.* (1988) evaluated a countermeasure used in Ontario, Canada, that combined a license suspension sanction with random spot checks of drivers. The countermeasure was the result of a law introduced in Ontario on December 17, 1981 permitting the police to conduct random spot checks to detect drinking drivers, and to suspend a drivers license for 12 hours if a driver registered 0.05 percent or more on a roadside screening device or evidentiary breath tester. The purpose was to provide a swift and certain punishment that would not tie up police or the court system. The authors note that celerity of punishment is one aspect of deterrence theory that has received little study. The study used a before-after design (January 1, 1979 - December 31, 1982) based on the proportion of fatally injured drivers with positive BACs among all those tested; comparison data from Saskatchewan - Manitoba were used. These data were analyzed by time series methods employing a Box and Taio intervention analysis. In addition, an assessment of print media coverage was made, telephone surveys of Toronto drivers were made before and after the law, and a questionnaire survey of police chiefs and uniformed officers concerning the law and its enforcement was carried out.

The fatality analysis revealed a small short-term effect. There was little media coverage, the telephone surveys indicated some awareness of the law but yielded no significant pre-post law changes in perceived enforcement, and the police survey indicated minimal enforcement of the law. The authors conclude that laws to increase the celerity and certainty of the punishment will have little deterrent effect without enforcement and publicity of the new laws.

Several studies have evaluated the effect of driver license restrictions for young drivers. Hagge and Marsh (1988) evaluated California's provisional licensing program for drivers 16-17 years old. The program went into effect on October 1, 1983, and required such drivers to complete additional parent-supervised driving practice; to wait longer after failing a written or driving test before retesting; and to be subject to postlicensing controls at lower negligent operator points. The effect of the law on both specific deterrence and general deterrence was assessed. The specific deterrence component examined driver records data from the California DMV. Basically, the design compared the subsequent accident and major traffic violation convictions of the pre-PDL group with that of the post-PDL group. The target group was compared to a control group of older drivers. The specific deterrence analysis found no significant effects on subsequent convictions or accidents.

The general deterrence component of the Hagge and Marsh evaluation studied several types of accidents, including those that are surrogates of alcohol-related accidents. Older drivers in California were used as a control, as were the same series in four other states. The analysis indicated a positive effect on all accidents among 15-17 year olds in California, but no significant effect on surrogates of alcohol-related accidents.

A report by Haque and Cameron (1987) documents an initial evaluation of zero BAC legislation in Victoria, Australia, which prohibits any learner (L), first-year probationary (P), unlicensed or disqualified driver or motorcycle rider from driving or riding with any alcohol in his or her blood. The introduction of the new law was accompanied by a PI&E campaign to promote awareness of the law and its penalties. The campaign included paid advertising as well as public service advertisements. A time series design was used in the evaluation, comparing the number of "serious casualty accidents" (SCA) of the experimental groups with the SCAs of two different control groups. (A SCA is an accident in which at least one person is killed, injured, or hospitalized.) Control group 1 was "standard license holders" (presumed to be drivers in general), and control group 2 was probationary drivers who had held their license more than a year. The authors considered both control groups not to be totally satisfactory, the first group because it included older, more experienced drivers, and the second group because it had a relatively small number of accidents. The second group of drivers may also have been influenced by the legislation because of peer influence and because they had previously been first-year probationary drivers. The main study examined four different time series of SCAs, target drivers at "alcohol times of the week" and "non-alcohol times of the week," and standard license holders at these two times of the week.

The study concluded that there was a reduction of about 4% in the number of learner, first year probationary, unlicensed and disqualified drivers involved in alcohol-related SCAs after the introduction of the zero-BAC legislation in Victoria. However, these results were not statistically significant, since the statistical tests conducted were only powerful enough to detect a change of 10%. This was because of insufficient post-intervention data.

Several states have passed curfew laws prohibiting young drivers from operating motor vehicles during late evening and early morning hours when many drinking-driving accidents involving this group occur. Preusser *et al.* (1983) examined the traffic safety effect of curfew laws on 16 year olds in four states, Pennsylvania, New York, Maryland, and Louisiana. These researchers compared reported crashes for a period of time before the law to those for a period of time after the law. The time periods studied were varied by state in order to ensure a large enough sample size. Data on crashes occurring during curfew hours and outside of curfew hours were collected for 16 year olds, older drivers, and drivers in comparison states. The study concluded that the laws substantially reduced the number of crash involvements of 16 year olds in all of the four states studied. These reductions amounted to 69% in Pennsylvania, 62% in New York, 40% in Maryland, and 25% in Louisiana. The effects of the laws were clearly greater in New York and Pennsylvania than they were in Maryland and Louisiana. Maryland law had a provision for an unrestricted license after six months of crash- and violation-free driving, and Louisiana's law was believed to have less awareness, compliance, and enforcement than the laws in New York and Pennsylvania.

Another study of Maryland's curfew law was conducted by McKnight, Hyle, and Albrecht (1983), at about the same time as the study by Preusser *et al.*, but with different results. The study by McKnight and associates also examined accident time series and found that the curfew had an effect on daytime accidents but no effect on nighttime accidents when some other factors were included (for example, a trend toward fewer daytime accidents). The authors' explanation of this result was that young drivers were required to have violation-free and accident-free record for a given period in order to obtain nighttime driving privileges and that this served as an incentive for safer daytime driving. This study also found that provisional licensing had a small but significant effect on daytime accidents.

In examining the above evaluations of drivers license sanctions conducted during the past decade, we were struck by the large number of well-designed studies in this group. As a result, some rather strong conclusions can be drawn about the highway safety impact of this group of countermeasures.

First, it is clear that driver license sanctions have a positive effect for a large group of convicted drunk drivers, including both first offenders and multiple offenders. There was evidence that license actions for *habitual offenders* imposed by administrative agencies may be more effective than those imposed by judicial agencies. Re-examinations of drivers with two or more "major" traffic violations (including DWI) within a three-year period and imposition of a license sanction was also found to be effective in reducing subsequent traffic law convictions. License actions were found to be more effective than treatment alone, but license actions plus treatment were more effective than license actions alone. There was also evidence that imposing license sanctions expeditiously through so-called administrative *per se* laws has a positive highway safety benefit, and that this benefit may be enhanced by an accompanying public information and education campaign.

### Jail

Two evaluations of this type of sanction have been conducted in recent years, one in Minnesota, and the other in Tennessee. The Minnesota evaluation by Falkowski (1984) examined the effect of a *policy* (not a law) adopted by Hennepin County, Minnesota judges to sentence all first-time DWI offenders to a two-day jail term. This was a multifaceted study and consequently had several target populations, namely convicted drinking drivers and the general population of Hennepin county. Both specific and general deterrence were assessed.

Different approaches were used in assessing the different impacts. Impact on traffic safety was evaluated by subjecting monthly night-time (10:00 pm - 4:00 am) injury accidents and DWI arrest data for Hennepin County and Ramsey County to time-series analyses. Traffic fatalities were assessed by a before-after, experimental-comparison design. The traffic data were analyzed using Box-Jenkins time series with 49 pre-policy and 17 post-policy data points. Both a bivariate model between DWI arrests and nighttime accidents and a final model which incorporated arrests, policy and nighttime accidents were used. A chi-square statistic was used to assess pre-post, experimental-comparison traffic fatalities, because the number of fatalities per month were too small to accommodate time series analyses

The author concluded that there was a statistically significant average monthly reduction of 35 night-time injury accidents two months after the adoption of the policy in Hennepin County. This is a 20% reduction when compared to the pre-policy monthly average. However, both the state and the comparison county had similar trends. We note also that there was a marked increase in the number of DWI arrests which was strongly related to nighttime injury accidents, and it is impossible to know whether the reduction in injuries was due to certainty or severity of punishment, or both.

The Tennessee evaluation was reported by Jones *et al.* (1988) and studied the effect of the adoption of a Tennessee law mandating a two-day jail term for first offenders and a 45-day jail term for second offenders. Though there appeared to be some reduction in nighttime fatal crashes after the intervention, the timing did not coincide with the intervention. Comparison with the control states, Alabama and Kentucky, showed some similar patterns. Thus, no clear evidence for an effect could be found.

They also examined recidivism patterns over an eight-year period and found a temporary reduction of 11% in recidivism rates after the law went into effect. However, this reduction was no longer evident three years after the law took effect. Although sanctioning patterns and jail records in two counties in the state indicated that the sanction was being applied to a large percentage of offenders (i.e., 80% or more), survey data from driver license applicants indicated relatively low awareness of the sanction and a general *perception* that it was not being applied to many offenders. Also, crowding of jails had the effect of increasing the time period between arrest and incarceration to as high as 18 months.

*Probation*

A study (Neff *et al.*, 1983) evaluated the effect of probation, rehabilitation, and probation plus rehabilitation on arrest recidivism of problem drinkers and non-problem drinkers in Mississippi over the period July, 1975, through June, 1981. The project was conducted in 11 small cities with a population of 8,875 to 46,264 and a "commitment to DUI enforcement and intervention." First offenders were allowed to retain their driver's license by participating in the program, a condition that would be likely to slightly inflate their recidivism.

The probation treatment consisted of the subject's reporting to a probation counselor once a month for 12 months for no longer than 30 minutes per session. For problem drinkers, rehabilitation consisted of a "modified form of group therapy" in which eight to 10 subjects reported for 90-minute sessions once a week for eight weeks. For non-problem drinkers, rehabilitation was attending four weekly 2 ½ hour sessions of a DWI school. In addition, about 100 subjects from each group were given the Current Life Status Questionnaire of the Life Activities Inventory (LAI). All entries into the various groups (not just those who completed their treatments) were retained for analysis.

The countermeasures evaluated were aimed at drivers assessed to be problem drinkers or non-problem drinkers assigned to the program. Initial screening was conducted at arrest, and assessment was made using a combination of the Mortimer-Filkins Questionnaire, prior record, and BAC at the time of arrest.

The research design involved random assignment to treatment and control groups and the examination of recidivism rate over a 24 month period following entry into treatment. Recidivism rates of the treatment and control groups were analyzed by using t-tests and contingency tables.

Neff and Associates found that there were no significant differences among any of the problem drinker groups. For the non-problem drinkers, the data suggested that probation might have been slightly better than non-probation, and that probation with rehabilitation might have been slightly better than probation alone. However, rehabilitation alone was clearly ineffective for problem drinkers or non-problem drinkers.

Eavy, Edwards, and Lee-Gosselin (1987) evaluated an experimental program in Michigan which required beginning probationary drivers who had accumulated two or more traffic convictions to appear before state authorities for a group re-examination. This study used a strong research design in which eligible subjects were randomly assigned to either the experimental group or to an untreated control group. Driving records of the two groups were compared at 6 months and 12 months following the treatment. The comparisons are made using t-tests of 6-month and 12-month accident and conviction rates, and 12-month mandatory suspension rates. The authors found that both 6-month and 12-month conviction rates were significantly lower for the experimental group, as were 12-month mandatory suspension rates. The experimental

group also had lower 6-month accident rates, but at 12 months there was no significant difference.

#### *Other Sanction Evaluations*

Three recent studies have examined the deterrent effect of several sanctions with divergent results. Ross and Voas (1989) compared the results of roadside surveys and studies of DWI recidivism patterns in two Ohio communities, New Philadelphia, where the judge imposed relatively harsh sanctions including jail, a high fine and license suspension, and Cambridge, where less severe sanctions were given. The study employed roadside surveys of nighttime weekend drivers to measure general deterrent effects of the much more severe penalties imposed in the experimental jurisdiction. Drivers' BACs were measured, and questionnaires were administered asking drivers about their perceptions of risk of arrest and severity of sanctions.

Though persons surveyed in the experimental jurisdiction consistently reported perceptions that the sanctions there were more severe, the BAC survey results indicated no difference between the jurisdictions, nor were differences in recidivism rates observed. Because their sample size was relatively small and other potential threats to validity were present, the authors were tentative about rejecting the hypothesis that severe penalties may deter impaired driving. For example, they pointed out that the stringent jail penalties were not consistently carried out because of a lack of jail space, that their roadside surveys were publicized and that a street fair in a nearby community may have differentially affected the survey results from the comparison community. Nonetheless, they hypothesized that if the deterrence model is valid, its application in their study, though elevating the perception of risk of arrest and subsequent imposition of severe sanctions, may not have had the desired effect because the actual risk of punishment was not high enough.

Joksch (1988) compared the fatal crash experience of seven states in 1980 through 1985 which he characterized as having adopted severe penalties (jail, community service, mandatory loss of license or combinations thereof) with seven other states. Joksch compared time series of the BAC distribution of fatally injured drivers in the jurisdictions with severe sanctions with the BAC distribution of fatally injured drivers in jurisdictions with less severe sanctions. He found that, although fatal crashes involving drivers with a high BAC were reduced in the experimental jurisdictions, there were similar reductions in the comparison jurisdictions. He qualifies his findings by stating that alternative explanations could be that earlier trends could have masked the effect in experimental states, that some comparison states could be voluntarily imposing similarly severe sanctions, that he had no measures of public awareness of the sanctions, and that lack of public awareness would make it difficult for the sanctions to have their desired effect.

Zador *et al.* (1988a, 1988b) examined fatal crash data during 1978 through 1985 from the 48 contiguous states to discern the effects of administrative license suspension/revocation, first offense mandatory jail or community service, and so-called per se laws (making driving with a BAC over a certain level a DWI offense per se).

Overall, they found all three strategies reduced driver involvement in fatal crashes, 2.4% for *per se* laws, 4.6% for license suspension, and 2.2% for mandatory jail or community service. Detailed tabulations by time periods with different levels of alcohol-involvement, and by driver and crash factors showed complex, sometimes unexpected, patterns. *Per se* laws were associated with a significant decrease in single passenger-vehicle crashes, and nearly always larger reductions at times when alcohol involvement tends to be low or moderate, than at times when alcohol involvement is usually high or very high. For administrative license suspension, and for mandatory jail or community service, the expected pattern appeared: reductions were larger at times when alcohol involvement is high or very high than when it is low or moderate. The authors employed alternative statistical techniques and obtained similar results. These findings are encouraging, but leave some questions unanswered. Among these is the effect of actual rather than prescribed adjudication and sanctioning practices. Actual practices often vary widely from those prescribed in statutes because of other laws with compensating effects or because of judicial discretion in imposing sanctions. For instance, Zador and associates included states allowing pre-trial diversion to treatment rehabilitation among the states with mandatory jail penalty, and no actual jail time had to be served for those so-diverted from the criminal justice system.

### COMPREHENSIVE APPROACHES

In the past 10 years, many states have revised their drunk driving laws, and some of these states have sponsored evaluations of these laws. Typically, these laws call for multiple countermeasures that employ more than one element of the Traffic Law System and have been evaluated as a package rather than as individual components. In some instances, statewide and even local programs have been implemented that also incorporate multiple countermeasures, and a few of these programs have also been evaluated. The evaluations of these more "comprehensive" countermeasure are discussed in this section.

One of the more extensive statewide programs of this type is the Stop-DWI program in New York state. The program and its evaluations are described in a series of reports by the Institute for Traffic Safety Management and Research (1985a, 1985b, 1985c, 1985d), Dowling (1986), and McCartt and Dowling (1985).

The evaluations were conducted by the Institute for Traffic Safety Management and Research in 1985 and 1988. They generally constitute an evaluation of this program for the first 30 months of implementation (December 1981 - May 1984). The STOP-DWI Program (i.e., "Special Traffic Options Program for Driving While Intoxicated") had four basic provisions: (1) Significant mandatory minimum penalties consisting of fine and/or jail for DWI and also driving while ability is impaired (DWA); (2) county programs developed and implemented by the counties, funded through the return of fines for alcohol-related offenses; (3) oversight of the local programs by the New York State Commissioner of Motor Vehicles; and (4) submission of an evaluation of the program. This program was unique in its extensiveness and also in the return of fines to the county.

The first four studies by the Institute for Traffic Safety Management and Research provide county-by-county evaluations of the programs with each county being introduced demographically followed by administrative and impact evaluations. The components of the administrative evaluation consist of enforcement, prosecution, adjudication, probation, rehabilitation, public information, education, evaluation, and administration. The components of the impact evaluation involve before and after analysis of accident experience. The final report by Dowling is a follow-up report (September 1986) consisting of an evaluation of the first four years of the program and is presented at a statewide level.

More specifically, the elements of the program and subsequent evaluation include the following: (1) documentation of the various program activities including incoming revenues as well as expenditures and time charts of the various county activities; (2) establishment of various immediate intervention programs and enforcement and adjudication efforts; (3) linking the results of the program to changes in public perception, knowledge, attitudes and behaviors with regard to drinking and driving as was done through mail and telephone surveys of New York State licensed drivers carried out in the Spring of 1983 and the Spring of 1984; (4) examination of the impact of the programs through before and after analysis of accident experience; a variety of surrogate measures were used such as fatal nighttime crashes, fatal single vehicle nighttime crashes, fatal single vehicle nighttime crashes involving male drivers, etc.; and (5) examination of alternative explanations to changes seen such as looking at factors like weather, "safety" factors, drinking age changes, economy, vehicle miles of travel, efforts of citizens' groups, national and regional crash trends, etc.

The impact evaluation was basically a before versus after study with some minimal attention paid to appropriate control groups. In making the before versus after comparisons, generally the baseline period of five years prior to December 1981 was compared with the subsequent period, namely, the first two years of the program and then also the six months in latter 1983 and early 1984. The six-month periods were compared with the corresponding six-month periods in the baseline. T-tests were used to determine differences between the before and after percentages. Log-odds ratios were used to try to incorporate control groups. Thus, log-odds ratios were used to compare nighttime fatal crashes to daytime fatal crashes in the before versus after period. In the statewide study by Dowling, a multivariate time series analysis was used to examine shifts in nighttime fatal crash experience.

The findings of the various STOP-DWI studies are summarized below.

Institute for Traffic Safety Management and Research (1985a) evaluated the county STOP-DWI programs in the metropolitan region: Nassau, New York City, Suffolk. It found that in Nassau, alcohol-related crashes as measured by injury crashes showed a significant drop following the STOP-DWI Program. In addition, the survey of attitudes showed an increase in knowledge and perceptions about drinking and driving. In New York City nighttime fatal crashes were down but there was no significant change in injury crashes. In Suffolk County, injury accidents showed a significant downward shift with implementation of the program.

The second report by the Institute for Traffic Safety Management and Research. (1985b) evaluated the county STOP-DWI programs in the north county region of New York state. It found a program impact in reducing alcohol-related crashes in the following counties: Clinton, Essex, Franklin, Lewis, St. Lawrence, Warren, and Washington. The program had ambiguous results in Jefferson and Oneida counties and showed no positive effect in Hamilton, Herkimer and Oswego counties.

The third report by the Institute for Traffic Safety Management and Research (1985c) evaluated the county STOP-DWI programs in the Hudson-Mohawk region. The study showed positive results on DWI-related crashes in Albany, Delaware, Greene, Montgomery, Rensselaer, Schenectady, and Schoharie counties; ambiguous results in Saratoga county; and non-significant results for Fulton county.

The impact evaluation of the STOP-DWI programs in the mid-Hudson region is reported in Institute for Traffic Safety Management and Research, 1985d. The study found indications of significant decreases in DWI-related crashes in Columbia, Orange, Sullivan, Ulster, and Westchester counties with ambiguous results presented for Putnam and Rockland counties.

Dowling (1986) updated and synthesized the other reports. The Dowling report covers the first four years of New York State's STOP-DWI county programs. The program period covered includes December 1981 through November 1985. Descriptive and statistical data relating to three of the components previously studied are provided including data on program activities, program expenditures and revenues, arrests and convictions, and fatal accident experience. Also provided are some data on changes in the caseloads of the probation and rehabilitation communities.

The impact analysis covered fatal accident experience, again using various surrogates for alcohol and non-alcohol related crashes. Analyses are presented for total fatal crashes, fatal nighttime crashes, single vehicle nighttime fatal crashes, single vehicle male driver nighttime fatal crashes, and weekend nighttime fatal crashes. Fatal crashes are also examined with respect to pedestrian crashes, fatal crashes per vehicle mile traveled and rural versus urban versus New York City area. The general conclusion is that, while daytime crashes dropped by seven percent during this four-year period, nighttime crashes fell by 27 percent. Dowling concludes that "of the analyses on the fatal crash experience, STOP-DWI continues to show success in reducing alcohol-related accidents and injuries in New York State."

Shore and Maguin (1988) evaluated the traffic safety effect of changes in the Kansas driving under the influence law. The new Kansas law went into effect on July 1, 1982. The law eliminated plea bargaining, adopted a *per se* limit for BAC, made refusal to take a chemical test evidence of DWI, and mandated jail penalties for multiple-offense DWI. The law was accompanied by a "large scale advertising campaign and extensive news coverage." The focus of the PI&E campaign was the plea bargaining provision and the "mandatory" 48-hour jail term for first offenders. (Jail was not really mandatory for first offenders because they could be diverted to a rehabilitation program and avoid jail.)

The evaluation analyzed a monthly time series of all fatal accidents using the SAS ARIMA procedure. The period studied was January, 1975 through December, 1983. The intervention function was a step-function with the step at July 1, 1982. Submodels incorporating unemployment, retail sales, and vehicle-miles travelled (VMT) were also used to account for other factors that might have influenced accident frequency during the period studied. No comparison states were used.

Shore and Maguin concluded that the new law (and, presumably, the accompanying PI&E program) was responsible for a 20% reduction in fatal accidents (eight per month), and that the effect prevailed throughout the 18-month period during which it was evaluated. The researchers found that there was no relationship between VMT and the reduction in fatal accidents, nor between either or both measures of economic conditions in the state.

They found a significant effect amounting to 8.1 fatalities per month less, after the intervention. Since the timing of the law coincided with the 1982 recession, they analyzed indication of economic activity: unemployment and retail sales. They found that the unemployment rate was related to VMT, but that neither VMT nor unemployment was related to fatalities. Neither were retail sales. Thus, they conclude that the entire observed effect was due to the law.

We compared Kansas fatality data with those in the neighboring Nebraska, and the entire United States and found that all follow the same pattern, suggesting that factors which had nationwide effects influenced the data. One such effect is the deep economic recession which occurred 1981-1983 (see Chapter 1 for a discussion of this factor). If there is any effect of the Kansas drunk driving laws, it must be much smaller than claimed by the authors.

An evaluation of the state of Washington's driving while intoxicated law was reported by Klingberg *et al.* (1984) and by Salzberg and Paulsruide (1984). The law mandated a one-day jail sentence for first offense DWI and established a *per se* limit. The law also required that first offenders attend a DWI school and authorized a suspended sentence of 180 days, conditional upon non-repetition. The authors appear to doubt whether the jail sentence was actually imposed and served by many offenders. The authors report that the jail requirement placed heavy burdens on the court system in the form of backlogs, delays in jailing offenders after their convictions, etc.

The study examined the recidivism of persons with a valid Washington driver's license, and who were convicted for 1st-offense DWI or multiple-offense DWI. The study used a before-and-after design with a comparison group. The before time period was 1978, and the after period was 1980. The comparison group was composed of drivers convicted of a non-alcohol offense during one of the two study years, and who had no alcohol-related convictions prior to that offense. Pairwise comparisons were made (before and after the law) for each of the three groups for recidivism as measured by alcohol-related violations, and for accidents. T-tests were used for comparing the per-subject means of the before and after groups.

The researchers found that recidivism in Washington actually increased slightly for all three groups. The increases were non-significant at the .01 level. Accidents rates increased for the two DWI groups, but decreased for the comparison group. Only the comparison group decrease was significant. The authors tempered the finding of no effect of the new law by speculating that the jail sentence may not have been imposed on many offenders; that enforcement increased overall after the new law; and that the 1978 offenders might have differed from the 1980 offenders because many of the high-risk offenders in 1978 had their charge reduced or were acquitted.

Jones (1985) performed an extensive evaluation of Oregon's new Drinking Driver Law, Senate Bill 710. The law went into effect on July 1, 1984, and contained provisions for failure of or refusal of a breath-alcohol test, a waiting period for issuing a provisional license during administrative suspensions, and an administrative *per se* law which allowed the arresting officer to confiscate the license of a driver who had failed or refused a breath test. The law also established a 48-day mandatory jail / community service sentence for a first-offense DWI, a mandatory alcohol evaluation, and mandatory treatment program for an alcohol / drug dependent person. The impact evaluation examined the effect of the law on motor vehicle deaths and serious injuries, and used an interrupted time series quasi-experimental design, employing Box-Jenkins ARIMA methods. Several measures were used, including all fatalities, alcohol-involved fatalities, nighttime fatalities, and various ratios. The time series studied covered the 1977-1984 time period. Fatality data were from FARS. No control states were included in the analysis. Jones concluded tentatively that the law probably reduced alcohol-related deaths and injuries. He noted that the effect was "certainly not overwhelming," a caveat that seems justified, especially given the level of statistical significance selected for the analyses, 0.20.

Another time series analysis was concerned with Minnesota's DWI law (Minnesota House of Representatives Research Department, 1985). The provisions of the law changes evaluated were classified as (1) those designed to facilitate apprehension and (2) those designed to prevent delay of license revocation. The group 1 changes allowed a warrantless arrest for DWI and eliminated the requirement for an officer to offer a blood test for a driver arrested for DWI. The group 2 changes allowed the administrative license revocation to remain in effect while the driver was awaiting a requested judicial hearing. The research design involved the use of time series of a number of surrogate measures of drinking-driving in three geographic regions. The measures were: (1) monthly counts of all fatalities, nighttime fatalities, and daytime fatalities, plus the ratio of nighttime to daytime fatalities; and (2) fatality rates per 100 million vehicle miles travelled for all fatalities, nighttime fatalities, and daytime fatalities. Attempts were made to control for seasonal variation and "historical patterns." The three geographic regions were the state as a whole, Hennepin County, and the state minus Hennepin County. Apparently, no non-Minnesota time series were used as controls, although the use of VMT as a denominator would be expected to account for some driving-related factors that might confound the results. Details of the method used by the Minnesota Department of Transportation in the computation of VMT (a critical issue given the lack of outside control groups) were not

provided in the report. The analysis applied ARIMA models to data covering an 81-month time period.

The study found that the policy changes reduced the number of fatalities in Minnesota by 18% and that the decreases found implied a 32% decrease in the overall incidence of drinking and driving statewide, a 28% decrease outside Hennepin County, and a 45% decrease in Hennepin County. These differences were attributed to the Hennepin County mandatory jail policy discussed elsewhere in this volume (See Falkowski, 1984). The report speculates that the positive effect was due mainly to the amendment preventing delay in license revocation and to the mandatory jail policy in Hennepin County.

A NHTSA project evaluated the effect of North Dakota's 1983 law which required prompt license suspension and mandatory sentences, and established an illegal *per se* standard (U.S. Department of Transportation, NHTSA, 1987). The mandatory sentence for first offense was relatively weak, requiring only a \$250 fine and referral to an addiction facility for evaluation. The "prompt license suspension" was actually an administrative suspension for having a BAC of .10% or more. The study analyzed various accident time series, including total crashes, total injuries, and total fatalities. The study used Box-Jenkins time series methods, both in SAS ARIMA version, and in a separate software package, AUTOBOX. Injuries and fatalities were broken down into daytime and nighttime. There were no control groups nor were there any terms in the series that might account for socio-economic influences known to effect traffic crashes.

The North Dakota study found a non-significant decrease in total crashes; no significant decrease in injuries; a significant decrease in fatalities; a significant decrease in single vehicle nighttime injuries; no significant decrease in single vehicle daytime injuries; a significant decrease in nighttime fatalities; and no significant decrease in daytime fatalities. The study states that these findings strongly support the conclusion that the new law had a positive impact on North Dakota drivers. While this may be true, other hypotheses about declines in the various types of crashes were not sufficiently explored to justify an attributing causal relationship to the law. The use of FARS data from nearby states and the use of other time series (e.g., unemployment) would have strengthened the study.

The last statewide evaluation we found (Hilton, 1983) assessed the effectiveness of some changes in California law dealing with drinking-driving. Hilton used an interrupted time series approach to evaluate the same law whose effect on DWI control system performance was assessed by Helander (1986a) and determined to be negligible. Using the ARIMA model, Hilton analyzed time series of various types of fatal accidents, including all fatals, daytime fatal accidents, weekend fatal accidents, and weekday fatal accidents. Hilton concludes that the new law was not responsible for the 12.9% statistically significant reduction in fatal crashes computed by the model. This conclusion was based on the finding that similar reductions occurred in other measures that could not be related to alcohol and drunk driving. More specifically, Hilton found that fatal accident levels for non-alcohol related accidents

declined about as much as fatal accident levels for alcohol-related accidents. He states:

“Rather than being alcohol-specific, the reductions in fatal accident levels have been across the board. This makes it difficult to attribute the 1982 reductions to the influence of the countermeasure package. Instead, it seems more likely that some unknown factor has caused reductions in all fatal accidents.” (p.11)

Nevertheless, Hilton does not completely rule out an effect, but goes on to speculate that the publicity attendant to the new laws plus the continuous publicity generated by advocacy groups such as Mothers Against Drunk Driving may have promoted safer driving in general, including a reduction in drunk driving.

We also found one evaluation of a local-level, comprehensive DWI countermeasure. This evaluation was conducted in Bonneville County, Idaho, by Amick and Marshall (1984). The program was described as “an integrated systems approach” to DWI. Called Project Safety, it began on October 1, 1979, and was evaluated over a 15-month period starting on that date. The program included a DWI selective traffic enforcement program (STEP) component; a probation and parole component; and a PI&E component. The enforcement component used two-man teams stressing nighttime / early morning enforcement at “high-DWI involvement.” Also, more patrol vehicles, a “direct” breath testing instrument, and videotape equipment were purchased. Presentence and probation staff were increased from two to five, and feedback on the status of offenders assigned to treatment. Inpatient and intermediate care services were provided “at the beginning of the project,” and outpatient and educational services were also provided. The nature of these services was not described. The PI&E campaign included radio interviews of DWIs, TV interviews on DWI, news releases, booths at community events, and presentations at local high schools. Nevertheless, Amick and Marshall report that media coverage decreased substantially after the first six months of the project.

The research design used a time series model with two control counties in Idaho. The impact measure was number of fatal and injury accidents occurring between 8:00 pm and 5:00 am. The time period examined was January 1975 through January 1981. Daytime fatal and injury accidents in the study counties and statewide were used as comparison series. The Box-Jenkins technique was used for analyzing the time series data.

The study found that there was a significant reduction in nighttime injury accidents in Bonneville County ( $p=.05$ ). The reduction started to occur one month after project inception, and amounted to 4.6 accidents per month compared to the pre-project level. Accidents in the one comparison county remained unchanged, and accidents in the other comparison county increased. Statewide accidents remained stable during the project period.

Finally, Votey (1984) evaluated the overall effect of drunk-driving legal-system control actions and alcohol consumption in two Scandinavian countries, Sweden and

Norway. Both time series and cross-sectional models were used. In Sweden, two time series were used, one describing "accidents" as a function drunk-driving convictions, per capita consumption of alcohol, and traffic density, and the other describing drunk-driving convictions as a function of total police manpower and accidents. The time series covered the period 1954-1977.

The cross-sectional studies in Sweden used data from 24 counties. Two separate analyses of these data were performed. The first analysis used data from 1972 and involved the use of four models. All models used fatal accidents, all motoring offenses prosecuted, and DWI prosecutions as independent variables. The first model used police patrol manpower, alcohol consumption, distance driven, and traffic density as dependent variables. The dependent variables for the second model were the same as the variables for the first model except that police patrol manpower was replaced by total police manpower. The dependent variables for the third model were the same as the variables for the first model except that alcohol consumption was replaced by cirrhosis of the liver for the year following the year studied. Finally, the dependent variables for the fourth model were the same as those for the third model except that police patrol manpower was replaced by total patrol manpower.

The second Swedish analysis used pooled data from the years 1971-1978 incorporated into a model with a single dependent variable, fatal accidents per capita, written as a function of total police manpower, per capita alcohol consumption, annual distance driven, and traffic density.

In Norway, two time series models were studied for the years 1956-1972, the first being concerned with personal injury accidents and the second with fatal accidents. Each model used four equations: convictions as a function of drunk driving, police manpower, and traffic density; drunken driving as a function of the probability of conviction and alcohol consumption; accident rate as a function of drunk driving, traffic density, and road quality; and demand for police patrol as a function of accident rate and per capita income.

Five cross-sectional models were used for Norway incorporating pooled county data for 1970-1975. The models were concerned with all injury accidents, fatal accidents, all accidents attributed to alcohol, fatal accidents attributed to alcohol, and all accidents, respectively. Each model incorporated two equations. The first equation wrote the pertinent dependent variable (for example, all injury accidents) as a function of prosecutions, license withdrawals, and an alcohol index. The second equation wrote the number of cases concluded for driver violations as a function of police patrol and the pertinent accident odds.

None of Votey's models incorporated any controls, since they were concerned with data for entire countries.

The general conclusion of Votey's study was that the study supports the hypothesis that increases in alcohol consumption are associated with higher accident levels and that increases in law enforcement effort leading to a greater probability of

sanctions will reduce fatal and serious accidents. The conclusion about the effects of law enforcement effort appear to be borne out by the results in both countries, but the conclusion about the effects of consumption are supported only by the Norwegian study. Some of the effects found were quite large, for example, a one percent reduction in accidents for a one percent increase in convictions in Sweden.

Summarizing and comparing the various evaluations of *comprehensive* countermeasure reviewed in this section is difficult because of possible confounding effects among the different interacting components of the various programs. Also, several of the programs evaluated were not really programs, but involved legislative changes whose degree of implementation was unknown. Of all of the various studies reviewed in this section, only those dealing with the state of New York's Stop-DWI program and Bonneville, Idaho's Project Safety were true programs. Both involved approaches employing enforcement, adjudication, sanctioning, and PI&E initiatives and were seem to have been adequately evaluated. Traffic safety benefits appear to have occurred.

The six legislative "programs" varied considerably in their provisions (see table below), and we found that only one program evaluation indicated a possible positive effect. Interestingly, that program was in Minnesota and had fewer of the "power" features than some of the other programs that did not show a positive effect. We speculate that this seeming anomaly might be due to the degree of implementation of the laws in various states, or to some other unknown factors that occurred at the same time as the laws.

Summary of Evaluations of Changes in State Laws

State	Provisions	Research Design and Findings	Comments
California	BAC per se limit. Stiffer penalties, including mandatory jail for multiple-offense DWI.	Time series with non alcohol comparison group. No effect.	Agree with findings. No effect.
Kansas	No plea bargaining. BAC per se limit. BAC test refusal evidence of DWI. Jail for multiple DWI.	Time series with VMT, unemployment, retail sales "control." Positive, significant.	Nebraska showed similar reductions.
Minnesota	No warrant for DWI. No blood test for DWI. License revocation remains pending hearing.	Time series with VMT "control." Positive, significant.	Some effect possible.
North Dakota	Mandatory \$250 and alcoholism evaluation for 1st DWI. BAC per se limit.	Time series with no comparison state. Positive, non-significant.	Probably no effect.
Oregon	Tougher rules for handling test refusals. Administrative per se law. Mandatory jail / community service for 1st DWI. Mandatory treatment for drug / alcohol dependence.	Time series with no comparison state. Positive, non-significant.	Probably no effect.
Washington	One-day jail for 1st DWI. BAC per se limit. DWI school plus 180 days suspended sentence for 1st DWI.	Recidivism with control groups. Negative, non-significant.	No specific deterrent effect.

SUMMARY AND CONCLUSIONS

Deterrent countermeasures use various elements of the Traffic Law System to catch and punish drunk drivers, thereby creating a perception that drunk drivers are at risk of being sanctioned. There is strong evidence that countermeasures that have stressed *enforcement* can have a significant traffic safety deterrent impact, particularly when used in combination with a strong public information and education (PI&E) component. Some of the stronger studies of the effects of enforcement coupled with public information campaigns found reductions in the number of nighttime accidents ranging from 10 to 30% (see summary table). However, some other strong studies found only small or "possible," but not significant effects. It is practically impossible to generalize such findings, because the effects depend on the intensity of the enforcement and the publicity campaign. Enforcement strategies employing the concept of sobriety checkpoints appear to have been successful in Australia, and some limited research in the U.S. suggests they may have been an important factor in some DWI programs that have combined enforcement with enforcement-strategy specific PI&E.

One *adjudicative* countermeasure aimed at increasing the probability that a charged drunk driver will be convicted of drunk driving (implied consent) was found also to have a traffic safety benefit in itself by suspending refusers' drivers licenses. Another adjudicative sanction, deferring prosecution as an incentive for entering a treatment program, was found to be ineffective.

Of evaluated countermeasures focussing on *sanctions*, those that suspended or revoked a DWI's driver license are clearly the most effective, particularly when well-publicized and applied administratively. One strong study showed that suspending the license of drivers refusing to submit to an alcohol test reduced their accident involvement during suspension, including alcohol-related accidents by about 70%, presumably because most did not drive while under suspension. Several other studies of different degrees of strength showed that suspending or revoking licenses for DWI reduced all accidents as well as alcohol-related accidents during the period of suspension or revocation. Comparisons among studies are difficult, because the sanctions were applied to drivers with different driving histories. There appeared to be no strong studies of effects beyond the period of suspension and revocation.

The impact of other sanction-directed countermeasures remains unclear. For example, one statewide study of incarceration in a jail reported no effect, while another local study of the same sanction reported a positive effect. In fact, there is still considerable controversy about the effect of sanction severity on drunk driving. Three studies reviewed here (Ross, 1987; Ross and Voas, 1989; and Joksch, 1988) suggest that the *severity* of a sanction may be less important than the *certainty* of a sanction, while another study (Zador *et al.*, 1988) found that certain severe sanctions (including jail) were highly effective.

Several studies have evaluated *comprehensive programs* employing multiple countermeasures. The results of these evaluations have been mixed. For example, the evaluation of the STOP-DWI program in New York state found that nighttime crashes fell by 27% during a four-year period while the program was operating, while daytime crashes dropped by seven percent. A study of changes in Kansas's DWI law and another study of changes in Minnesota's DWI law also found a positive effect statewide. On the other hand, a strong evaluation of changes in California's DWI laws concluded that the 12.9% statistically significant reduction in fatal crashes computed by his model were not due to the law but were more likely due to "some unknown factor." Evaluations of law changes in North Dakota, Oregon, and Washington indicated no significant effect. None of the evaluations of statewide programs used other states as a control, leaving claims of effectiveness somewhat questionable.

Summary of Evaluations of Enforcement Countermeasures<sup>a</sup>

Reference	Description	Design	Findings
Calderwood and Woods, 1983	BATmobiles in Albuquerque, NM.	1,3,4	Concluded that the program reduced accidents, but data were insufficient for predicting the amount of the reduction.
Homel <i>et al.</i> , 1988	Random breath tests with a PI&E program in New South Wales, Australia.	2	Found a 33% reduction in number of fatally injured drivers with a BAC of .05% or more.
Lacey <i>et al.</i> , 1986	A large-scale, multi-year enforcement / PI&E program in two adjacent FL cities.	1,3,4	Found a significant intervention effect of 13%-20% on nighttime crashes.
Lacey <i>et al.</i> , 1988	Similar program to the previous study. Test site was Indianapolis, IN.	1,3,4	Found only a possible effect due to a less intense PI&E campaign.
Levy, Shea, and Asch, 1989.	Sobriety checkpoints combined with an education campaign in New Jersey.	1,4	Found that the checkpoints decreased nighttime single-vehicle crashes by 10%-15%.
Lynn, 1985	13 selective enforcement programs in VA.	2	Ten of the 13 localities met their stated goals in terms of alcohol-related accidents. [Study not conclusive because of lack of controls.]
McLean, 1984	Random breath tests in South Australia.	Road-side Surveys	Found a 14% reduction in percentage of drivers at or above a BAC of .08%. Effect disappeared within a year.
Pigman and Agent, 1986.	A combined enforcement / PI&E program in Lexington-Fayette County, KY.	2	Found a 30% reduction in reported alcohol-related accidents due to the program. [Some effect is likely, but may be less than found because of possible time trend.]
Ross, 1987b	A one-month combined enforcement / PI&E program in England and Wales.	2	Found a 23% reduction in all fatalities during the month the program was in effect. [Apparent lack of a control groups makes finding questionable.]
Ross, McCleary, and Epperlein, 1982	Random breath tests in France.	1,3	Found a temporary reduction in crash related deaths and injuries. Few arrests made at the checkpoints.

Reference	Description	Design	Findings
Vingilis, Chung, and Adlaf, 1981	A two-year combined enforcement / PI&E program in the Toronto, Canada area.	1,3,4	Found no measurable highway safety effect.
Voas, Rhodenz, and Lynn, 1985.	A one-year sobriety checkpoint operation in Stockton CA.	1,3,4	Found a significant 15% decrease in alcohol-related accidents in Stockton. Decrease was not significant when compared with decrease in the rest of the state.
Voas and Hause, 1987	Increased patrol activity in Stockton, CA.	1,3,4	Found significant 10%-15% decreases in nighttime accidents due to the countermeasure.
Wolfe and O'Day, 1984	A 4-year combined enforcement / PI&E program in Oakland County, MI.	2	Alcohol-related accidents decreased, but no control groups were reported. [Thus, the actual effect of the program is unknown.]

a. Codes for research design are: 1 - time series; 2 - other, non-time series; 3 - control jurisdictions; 4 - other controls.

Summary of Evaluations of Adjudication and Sanctioning Countermeasures<sup>a</sup>

Reference	Description	Design	Findings
Amick and Marshall, 1984	Comprehensive DWI program in Bonneville County, ID.	1,3,4	A significant decrease in nighttime injury crashes.
Blomberg, Preusser, and Ulmer, 1987; Preusser, Blomberg, and Ulmer, 1988	Wisconsin's law mandating 3 to 6 month driver-license suspensions for 1st offense DWI.	1,4,7,9	A 25% reduction in alcohol-crashes (from general deterrence analysis). 50% reduction in subsequent DWIs after first six months of program (specific deterrence analysis).
California State Department of Motor Vehicles, 1986	Warning letters and informational materials sent to DWI first offenders in California.	5,6,9	Warning letters were not effective in reducing subsequent DWIs or accidents.
Eavy, Edwards, and Lee-Goselin, 1987	A Michigan program requiring a group re-examination for beginning probationary drivers with two or more traffic convictions.	5,9	Fewer subsequent convictions and accidents among the experimental group.
Falkowski, 1984	Mandatory jail policy in Hennepin County, MN.	1,3,4	A 20% reduction nighttime injury crashes due to the policy.
Hagge and Marsh, 1988	California's provisional licensing program for age 16-17 drivers.	1,3,4,9	General deterrence component found a positive effect of the program on all accidents, but no significant effect on alcohol crashes. Specific deterrence component found no effect on subsequent accidents or convictions.
Hagen, McConnell, and Williams, 1980	Driver license suspensions in California.	6	Suspensions are effective in reducing subsequent accidents and DWIs involving first and multiple offenders.
Haque and Cameron, 1987	Victoria, Australia law prohibiting learner and probationary drivers from driving with a BAC > 0.	1,4	A 4% non-significant reduction in serious casualty accidents among the target group due to the law. There were insufficient data to show an effect of less than 10%.

Reference	Description	Design	Findings
Helander, 1986b	Habitual traffic offender law in California (charge rates and conviction rates).	NA	Because of concern for double jeopardy, prosecutors only charged 4% of those meeting HTO criteria. Only 1% were convicted.
Hilton, 1983	Changes in California's DWI law (See Helander, 1986a).	1,4	No effect compared to non-alcohol crash surrogates
Institute for Traffic Safety Management and Research, 1985a,....,1985d; Dowling, 1986; McCartt and Dowling, 1985	New York State's comprehensive STOP-DWI program.	2,4	Program generally showed an effect overall and several various counties where it was implemented. Nighttime crashes dropped 27% compared to 7% for daytime crashes in New York.
Joksch, 1988	The effect of severe sanctions in 7 states that had laws requiring them.	1,3,4	No effect of severe sanctions on fatal crashes involving drivers with a high BAC.
Jones, 1985	Oregon's comprehensive DWI law.	1,4	Non-significant decrease in alcohol-related traffic deaths.
Jones, 1986	Driver license revocation for habitual offenders in Oregon.	9	Revoked drivers had lower subsequent high-risk traffic violations (including DWI).
Jones <i>et al.</i> , 1988	Mandatory jail law in Tennessee.	1,3,4, 6,7,9	No significant reduction in alcohol-crash surrogates. Temporary reduction of 11% in reduction of DWI recidivism.
Kadell and Peck, 1982	Re-examination of California drivers with two or more major violations (including DWI) with possible driver license action.	5,6,9	Significant reduction in reconvictions due to the program. The program was deemed probably cost-effective.
Klingberg <i>et al.</i> , 1984; Salzberg and Paulsruide, 1984	Washington state's comprehensive DWI law.	6,9	A non-significant <i>increase</i> in subsequent alcohol-related accidents for experimental and control groups. [Lack of adequate controls may have contributed to this negative result.]
Lacey <i>et al.</i> , 1989	Adoption and awareness of an administrative <i>per se</i> law in Nevada.	1,4	A 10% reduction in nighttime crashes and a further 7% after the law was publicized.

Reference	Description	Design	Findings
Marsh, 1987	California's habitual offender program called the Negligent Operator Program.	5,6,8,9	Program effect was not significant for the DWI subgroup, but was positive and significant for the HTO subjects overall.
McKnight, Hyle, and Albrecht, 1983	Curfew law in Maryland.	1,4	No significant reduction in nighttime accidents among target group (compared to 25% for study by Preusser <i>et al.</i> ).
Minnesota House of Representatives Research Department, 1985	Minnesota's comprehensive DWI law.	1,3,4	An 18% reduction in the number of fatalities in the state due to the law.
Neff <i>et al.</i> , 1983	Probation versus rehabilitation in Mississippi.	5,9	No significant effects of the sanctions for problem drinkers. Rehabilitation alone not effective for problem drinkers or non-problem drinkers.
Preusser <i>et al.</i> , 1983	Curfew laws in four states.	2,3,4	Laws reduced reported crashes among target group during curfew by 25%-69%.
Ross, 1987a	New Mexico's administrative <i>per se</i> law.	1,4	A 10% decline in the percentage of drivers and pedestrians with a BAC of .05% or more.
Ross and Voas, 1989	The effect of severe sanctions in a small town in Ohio.	Roadside surveys, 3,7,8	No general deterrence or specific deterrence effect. Had small sample size.
Sadler, 1986	California's implied consent law which required driver license suspension for refusing a BAC test.	6	Suspending driver license of breath test refusers can be an effective countermeasure. Suspended refusers had 64% fewer alcohol-related crashes than non-suspended refusers.
Sadler and Perrine, 1984	Comparison of license suspension with treatment for multiple offenders in California.	6,7,8,9	Treatment group had 9% fewer alcohol-related convictions than suspension group, but treatment group and suspension group had the same number of alcohol-related crashes.
Salzberg and Klingberg, 1981	Driver license revocation for male habitual offenders in Washington state.	9	License revocation is an effective countermeasure, and there was no evidence that stay of revocation or treatment was effective.

Reference	Description	Design	Findings
Salzberg and Klingberg, 1986	Washington state's deferred prosecution law allowing charge dismissal after completion of treatment.	6	No positive effects of the program, and a possible negative effect.
Shore and Maguin, 1988	Kansas's comprehensive DWI law.	1,4	A 20% reduction in fatal accidents due to the law. [Similar reduction in Nebraska.]
Tashima and Peck, 1986	Comparison of license suspension and license-restriction-plus-treatment in California.	6,9	Suspension more effective than restriction-plus-treatment for multiple offenders. For 1st offenders, treatment more effective in preventing alcohol-related crashes; suspension more effective for all crashes.
U.S. DOT, NHTSA, 1987	North Dakota's DWI law whose main feature was administrative <i>per se</i> .	1,4	A non-significant decrease in various alcohol-crash surrogates.
Vingilis <i>et al.</i> , 1988	License suspension and random spot checks in Canada.	2,3,4	A small, short-term effect was attributed to the program.
Votey, 1984	Overall legal-system control actions in Sweden and Norway.	1,2,4	Increases in alcohol consumption are associated with higher accident levels. Increasing certainty of sanctions will reduce fatal and serious accidents.
Williams, Hagen, and McConnell, 1984	Driver license suspensions in California.	9	1st offender DWIs without suspensions had generally higher recidivism than multiple offenders with suspensions.
Zador <i>et al.</i> , 1988a; 1988b	The effect of severe sanctions in all of the contiguous 48 states that had them.	1,3,4	2.4% reduction in fatal crashes for administrative <i>per se</i> ; 4.6% for license suspension; 2.2% for mandatory jail or community service.

- a. Codes for general deterrence research design are: 1 - time series; 2 - other, non-time series; 3 - control jurisdictions; 4 - other controls. Codes for specific deterrence research design are: 5 - random assignment to treatment and control; 6 - Non-random assignment, covariance analysis; 7 - other non-random assignment; 8 - control jurisdictions; 9 - other controls.

## CHAPTER 4 - TREATMENT AND REHABILITATION

This class of countermeasures is targeted at dysfunctional drinking related to drinking-driving. Rehabilitative educational programs are included in this category. As we have noted elsewhere in this report, nearly all of these countermeasures are operated in conjunction with those that use the Traffic Law System to deter and incapacitate drunk drivers (see Chapter 3). They are more related to so-called specific deterrence countermeasures than to general deterrence countermeasures, because they require that a drunk driver first be brought to the attention of the treatment and rehabilitative agents. The enforcement and adjudicative components of the Traffic Law System typically act as the case finders in this process.

### DISCUSSION

Blount (1983) evaluated several different rehabilitation programs conducted by an organization in Hillsborough County and Pinellas County, Florida. The author attempted to identify problem drinkers and social drinkers prior to assignment to the various treatments. There was also an attempt at random assignment to the programs through agreements with judges. Those found to be social drinkers were assigned to either a "read only control" group and given a series of materials to read at home, or to a DWI school and social-drinker class modeled after the "Phoenix Plan" of Stewart and Malfetti. The course consisted of four 2 ½ hour sessions given at the rate of one session per week. About 20 students attended each class. The problem drinkers were assigned either to a read only control group; a DWI school (apparently similar to the DWI school for social drinkers with same number and length of sessions) and problem-drinker class (with factual material similar to that used in the social drinker class, but with emphasis on different topics); or a group which added group therapy (five months at one hour per week) to the DWI school / problem-drinker class.

Comparisons were made between groups with respect to subject characteristics, treatment assignments, and course completion, with essentially no significant differences emerging. For example, program completers in the various groups had about the same characteristics as non-completers. DWI arrest recidivism was tracked for up to 18 months from assignment and / or completion. A survival analysis approach was used for analyzing recidivism.

Blount concluded that drinking driving behavior seemed to have been changed for both the social drinker and the problem drinkers. For the social drinker group, the data suggest that the treatment reduced 18-month recidivism by 35%, from .121 to .079 ( $p = .01$ ). For problem drinkers, the group completing DWI school + class + group therapy had an 18-month recidivism rate of .060 compared to .145 for the control group (no level of significance indicated). However, only 55% of the problem drinkers assigned to the two treatments completed their treatment.

Reis (1982) reports the results of an extensive evaluation of a large-scale education program for first-offense drunk drivers in Sacramento, California. The specific deterrent effect of a home-study

course and an in-class course was evaluated over a two-year period. The home study program consisted of an organized set of reading materials designed as a self-study, self-paced package. The program was introduced to the clients in a one-hour session which included a pretest. The client returned in four weeks for a five-minute interview and a post-test. The in-class program consisted of four weekly sessions of 2 1/2 hours each with the same content as the home-study course. Each class had about 18 clients. It was described in the report as a more or less typical alcohol education program being used in conjunction with court treatment programs at the time (*circa* 1980).

Reis used treatment and control groups with random assignment. Care was taken to ensure that the assignments were truly random. Each of the three groups contained approximately 1,500 subjects which were tracked for up to 22 months, depending upon how near to the end of the project they began the program. Traffic safety outcome measures involved subsequent convictions for DWI and reckless driving (because of the common practice of charge reduction), subsequent alcohol-related accidents, and subsequent non-alcohol related moving violations. Two submeasures were used for the accidents, the first measure being a police-reported alcohol-related accident or a nighttime injury/fatal accident or a nighttime single-vehicle accident. The second measure was police-reported alcohol-related accident or a nighttime accident or an injury accident. Non-alcohol related moving violations were used as a control measure. The evaluation used a variety of techniques based on the survival-analysis approach.

Reis concluded that the education program was effective, reducing the 1-year rearrest rate for DWI from 14% to 12%, the 2nd-year rate from 23% to 20%, and the 3rd-year rate from 28% to 25%. There was no effect on non-alcohol related moving violations. No differential rate was found for the home-study or the in-class programs. However, no effect was found for alcohol-related accidents, a result that was attributed to the small number of subsequent accidents. Also, the evaluation found no evidence of program-induced changes in client life status.

Another study by Temer *et al.* (1987) suggested positive results, but cannot be accepted unequivocally because of design problems. These researchers examined post-treatment DWI recidivism and major-offense recidivism of 1,914 persons referred by the court to an outpatient treatment program conducted by Occupational Health Services, Inc., of Oakland, California, during the period January 1981 through February, 1983. The program had a mandatory one-year attendance requirement. The program components were 13.5 hours of educational classes; 58.5 hours of group therapy; eight hours of individual counseling; and attendance at 20 Alcohol Anonymous (AA) meetings. In addition, Disulfiram was required of all participants not contra-indicated entering the program before March 8, 1982. After that date, participants could choose between the Disulfiram and an additional 24 AA meetings.

The study compared records of those completing the program to those not completing the program. The reasons for non-completion were not discussed, but analyses showed no correlation between completion or recidivism and several socio-economic and alcohol use items. Nevertheless, one cannot be reasonably sure that the completers were not somehow different from the non-completers. Analysis of variance methods were used in the analysis.

Temer and associates concluded that those who completed the program had a lower recidivism than those who did not complete the program, and that the recidivism rates of those who attended AA and those who took Disulfiram were about the same. (Note that it not necessarily due to the

program but could be due to different types of persons in the completer and non-completer groups.) Further, those who had the opportunity to choose between treatments did better than those who could not choose their treatment. However, among those given the opportunity to choose between treatments, the AA group did better than the Disulfiram group. Interestingly, the authors concluded that it was act of being given the right to choose was what made the difference, rather than the type of treatment that was responsible for the effectiveness of the program.

Many other evaluations of treatment and rehabilitation countermeasures have been less positive than those discussed above. For example, Holden (1983) evaluated the Tennessee DWI Probation Follow-Up Demonstration Project in which 4,126 Memphis first-offense DWIs categorized as problem drinkers or social drinkers (using the Mortimer-Filkins test) plus BAC at the time of arrest were randomly assigned to one of several treatments, viz.: probation supervision, education / therapy, or supervision plus education / therapy. A control group consisting of convicted DWIs with no conditions of probation was also used. Persons were assigned to the project as a condition of probation, either after conviction or on a judgement-withheld basis. Social drinkers attended only the education program (basically, a DWI school) of the education / therapy treatment; problem drinkers attended both the DWI school plus an assertiveness training program consisting of eight 1.5 hour group therapy sessions. Subjects assigned to supervision had to report to their probation officer once a month for a half-hour meeting for a period of one year.

The research design incorporated a  $2 \times 2$  factorial randomized experimental design. Each subject was followed for a minimum of two years, one year in the program and one year after the program. Rearrest rates for DWI and for other misdemeanors and felonies were the measures of effectiveness. The analysis used failure analysis techniques with a so-called proportional hazard rate model.

Holden concluded that none of the treatments had any significant effect on DWI recidivism, either for social drinkers or problem drinkers. However, one treatment, supervision, had a significant effect on non-DWI recidivism for problem drinkers. Data indicate that nearly all of the subjects completed their treatment. Holden observes that the classroom settings were probably inappropriate for the subjects: 41% had less than a high school education, and 71% reported family incomes of less than \$12,000. Also, many clients had prior criminal records and were not likely to be affected by such treatments. Further, the treatments themselves were relatively weak. Finally, the author concludes that the "sanction" of being arrested and made subject to traffic law system procedures may have been a stronger sanction than the treatments and could very likely have swamped any treatment effects.

Stewart *et al.* (1987) report the results of a four-part study of a model DUI first-offender rehabilitation program in California.<sup>4</sup> The study involved preparing a detailed system description of the health/legal system for first offenders in 56 counties; developing and implementing the model program; evaluating the model program in two counties; and conducting a management study of the program. According to the authors, the program "resembles traditional programs in many respects but has several important features that distinguish it from most other programs." The first of these features was a two-step approach consisting of a six week educational program followed by a seven week counseling program. A second feature, claimed to be unique, was having each subject prepare

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<sup>4</sup> See also Stewart, Gruenewald, and Roth, (1988).

specific strategies for avoiding that person's drinking-driving situations, and a third unique feature was assessing a subject's drinking and other life problems and referring those subjects to appropriate community resources.

The target group was all DUI first offenders. A breakdown of this group by some demographic variables (for example, age and sex), driving variables, and drinking variables was given. A sample of about 50% of the subjects in the two case-study counties were interviewed, but the results of the interviews were presented only in general terms (for example, "Sex ratios and age were similar at the two sites." We note that this study provides a much better description of the target population than that found in most evaluation studies.

The basic research design was strong, employing random assignment to treatment and control groups. Four groups were involved: (1) the traditional program in the county; (2) a six-week education-only model program; (3) the 13 week education-plus-counseling program; and (4) a community service "control" group with minimal programmatic content. Departures from a pure random assignment procedure (as described) were minimal. The only real weakness of the design was the relatively small sample sizes for the various groups, ranging from 181 for the education-only group to 340 for the traditional group (total N=971). This results in an inability to detect effects of a magnitude that might be expected from an intervention of this type. Another weakness is the short follow-up period (only five months for those entering the program last) that was available for the recidivism analysis.

Outcome measures were frequency and quantity of drinking, number of drinking-driving incidents in the past nine months, and frequency of drinking-driving compared to that of others, and whether or not the subjects had driven within two hours of their last drinking episode. ANOVA methods were used for the study of drinking and drinking-driving patterns, and a probit analysis was used for the study of driving after two hours of drinking, and for analyzing recidivism rates.

Stewart and associates found no significant differential program effects of drinking patterns and drinking-driving, but concluded that there was a small decline in heavy drinking and drinking-driving regardless of type of program. Because the differences between programs were so small, the authors concluded that the lack of differential effects was probably not due to small sample size. Also, no differential program effects were found in arrest recidivism, but this finding was inconclusive because of the short follow-time available for tracking subjects.

LeClair, Felici and Klotzbier (1987) evaluated the use of prison confinement for the treatment of multiple drunken driver offenders. The facility, Longwood Treatment Center in Massachusetts, was a minimum security prison designed exclusively to detain and provide alcoholism education and treatment to multiple drunken driving offenders. The study involved a comparison of the reincarceration rate of DWI offenders treated at the special facility with that of offenders handled through the normal prison procedures. The critical findings were "Our research demonstrated that 6% of the Longwood program completers were returned to prison within one year of release. This compares to a department wide recidivism rate of 25% and to a rate of 19% for other low security institutions similar to the Longwood program."

This finding would be highly indicative of a successful program were it not for the fact that offenders assigned to Longwood were highly selected. Of 562 OUI offenders committed to the house

of corrections in Dedham and Billerica, 86 (15%) were transferred to Longwood. After screening according to various criteria, 117 (26%) were found eligible, but 31 were not interested in transferring. The remaining were transferred. Further, there were differences between the Longwood population and the OUI population in county houses of correction: the percentage of females was much higher, the mean age was higher, the educational level was higher, and more were single.

To measure the outcome, the authors report that 305 residents released from Longwood were examined. Forty-two were "program failures" (returned to county house of correction, or escaped); only the remainder were included in the evaluation; 263 were followed for six months, 174 for nine months, and 99 for 12 months. During these periods, 4%, 13%, and 11% had at least one subsequent arrest for DUI, respectively. Of the last group, 6% were re-incarcerated during the 12 months of follow-up.

Thus, since the population studied was highly selected, and finally volunteered, the results cannot be taken at face value. Also, the recidivism rates of 25% overall, and 19% for other minimum security prisons with which the Longwood rate is compared are not characterized in detail, for example, for which offense. Therefore, the comparison may not be valid. The lack of random assignment or any apparent covariance analysis makes the findings of this study less than conclusive.

Our review of driver-license countermeasures in the preceding chapter indicated that many of the treatment and rehabilitation programs for first offenders have been used in lieu of hard license suspension. That is, offenders have been "diverted" from receiving real licensing sanctions into education or treatment programs. In fact, it is the evaluation of the effectiveness of such programs that gave early indications of the effectiveness of license suspension or revocation in reducing recidivism. The more recent California studies discussed above confirmed this finding and also found that rehabilitation, in addition to license suspension, was more effective than license suspension alone.

Another recent study by Popkin, Stewart, and Lacey (1988) indicated that *educational programs when implemented in addition to licensing sanctions*, may also have some traffic safety effect. It found that, after controlling for other factors related to DWI recidivism (for example, age and BAC at time of arrest), there was a small, statistically significant benefit as a result of attending the course. Although the main focus of the study was recidivism, no effect was found on crashes. These findings conflicted with their earlier study of the effect of a similar program which, in effect, served as a diversion from a hard license suspension (Popkin, *et al.*, 1983). In that study, persons who attended the educational program and thus were entitled to restricted driving privileges, had higher recidivism rates than those who did not attend the program and received a hard license suspension.

Some unexpected results have emerged from evaluations of treatment and rehabilitation countermeasures. For example, the study by Neff *et al.* (1983) (discussed under *Probation* sanctions above) evaluated the effect of probation, rehabilitation, and rehabilitation plus probation, on DWI recidivism. A group receiving none of these was used as a control. These researchers found that the administration of the Life Activities Inventory, an instrument used to assess intermediate measures of program effectiveness, had an effect on DWI rearrest rates for persons defined as non-problem drinkers, but that the interventions actually being evaluated had no such effect.

A number of Canadian treatment and rehabilitation countermeasure programs have been evaluated, many of them before the time period covered by this review. Liban, Vingilis, and Blefgren (1987) reviewed seven such programs and concluded that Canadian rehabilitation programs have had some positive effects on knowledge, and mixed effects on attitudes and traffic safety measures. There were problems in implementation (usually not discussed in the evaluation studies), including hostility and intoxication among participants resentful of their assignment to the course. The authors speculated that the limited traffic safety impact of the programs may have been due to methodologic shortcomings in the evaluations.

Finally, we note that evaluative research in the area of treatment and rehabilitation countermeasures for drunk driving has been blessed by several outstanding review articles, some of which cover a time period extending further into the past than this review. In addition the review by Liban and associates cited above, these include reviews by Nichols *et al.* (1981); Mann, *et al.* (1983); Hagen (1985); Foon (1988); and Mann *et al.* (1988).<sup>5</sup>

The latest review by Mann and associates is of particular interest. These researchers reviewed evaluations of the effectiveness of treatment and rehabilitation programs with respect to design, assessment instruments, follow-up procedures, and results. They concluded that several of the programs evaluated may have reduced recidivism, and note that only 15 rehabilitation evaluations have used control groups with random assignment of subjects, and only six evaluated traffic safety impact. The study gives a detailed discussion of major research design issues (lack of control groups and random assignment, and failure to use impact measures), and then reviews the evaluations with respect to these issues. The authors discussed the difficulty of using impact measures (e.g., need for large samples because of small effects of most programs) and concluded that multiple measures are needed. They pointed out the flaws in using recidivism alone as a follow-up measure (i.e., its dependence upon criminal justice system actions), and gave supplemental measures (e.g., treatment / lifestyle measures). The authors believe that past rehabilitation programs may have been more effective than their evaluations showed, because of poor research designs, etc.

## SUMMARY AND CONCLUSIONS

The evaluations reviewed in this study do not provide strong support for the hypothesis that alcohol-related crashes can be reduced by treatment and rehabilitation, although two strong studies found reductions in the re-arrest rate ranging from 10 to 35%. This conclusion applies to programs that deal with social drinkers and first offenders as well as to programs that deal with persons with drinking problems and with multiple offenders. Further, there appears to be a disturbing tendency for the better designed and executed evaluations to show little or no impact, and for the less rigorous evaluations to show an impact. Nevertheless, more recent studies continue to confirm past studies indicating that rehabilitative sanctions can be effective when applied in addition to traditional sanctions such as driver's license suspension or revocation.

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<sup>5</sup> A bibliography of pertinent review articles may be found at the end of this volume.

Summary of Evaluations of Treatment and Rehabilitation Countermeasures<sup>a,b</sup>

Reference	Description	Design	Findings
Blount, 1983	DWI schools and group therapy in two counties in Florida.	5,9	Social drinkers sent to DWI school had their recidivism reduced by 35%. Recidivism of problem drinkers given school and group therapy had a recidivism 60% lower than control's. Only 55% of test groups completed the course.
Holden, 1983	Various combinations of probation, therapy, education, and supervision in Memphis TN.	5,9	None of the treatments had any significant effect, either for social drinkers or problem drinkers.
LeClair, Felici, and Klotzbier, 1987	Prison confinement for the treatment of multiple offenders.	7	Experimental group had a lower recidivism rate than those not assigned to the program. [Assignment was highly selective.]
Neff <i>et al.</i> , 1983	Probation, rehabilitation, and probation plus rehabilitation in Mississippi.	5,9	The various interventions had no effect on DWI recidivism.
Popkin, Stewart, and Lacey, 1988	DWI schools in North Carolina.	6	Small effect on recidivism; no effect on crashes.
Reis, 1982	Education program for first offenders in Sacramento, CA.	5,9	Reduced DWI recidivism 2-3 percentage points, but had no significant effect on accidents because of small sample size.
Stewart <i>et al.</i> , 1987	Several education and counseling programs in California.	5,9	No differential effects among various approaches studied. Possible decline in drinking-driving for all types of approaches.
Temer <i>et al.</i> , 1987	A 1-year education and treatment program (including AA and Disulfiram).	6	Program completers had a lower recidivism than non-completers. No difference between AA and Disulfiram groups. [Possibly confounded by lack of controls.]

- a. Codes for specific deterrence research design are: 5 - random assignment to treatment and control; 6 - Non-random assignment, covariance analysis; 7 - other non-random assignment; 8 - control jurisdictions; 9 - other controls.
- b. Two cites from the summary table on adjudication and sanctioning also apply to this table. They are Sadler and Perrine (1984) (page 56) and Tashima and Peck (1986) (page 57).

## CHAPTER 5 - CONCLUSIONS AND RECOMMENDATIONS

Three major categories of drinking-driving countermeasures were reviewed in this project:

- Regulating the availability of alcohol - Use of the legal system to control access to alcoholic beverages.
- Deterring and incapacitating drunk drivers - Use of the Traffic Law System to create a high perception of punishment for drunk driving and to deny access to a motor vehicle.
- Treating and rehabilitating drunk drivers - Use of education and treatments to prevent dysfunctional drinking related to drinking-driving.

In this country, *regulating the availability of alcohol* has been attempted primarily through raising the legal minimum drinking age. At this writing, the legal minimum drinking age is 21 years in all states. Several excellent evaluations of the impact of this countermeasure have been conducted and present overwhelming evidence that it has reduced alcohol-related fatalities. Our review of the strongest studies shows that fatal accidents involving drivers of the affected age groups were reduced by between 9 and 14%. Other strategies for regulating alcohol availability have received far less attention by evaluators. One strong study of relaxing restrictions by allowing selling liquor by the drink for on premise consumption found that alcohol related accidents increased by 10-15% in the affected counties. Evaluations of attempts to restrict the sales of alcoholic beverages through such countermeasures as banning happy hours have found no significant traffic safety impact. The possible traffic safety benefit of increasing taxes on alcoholic beverages has not really been evaluated, although some studies suggest such an initiative may have potential and deserves further study.

Evaluations of *deterrent and incapacitive* countermeasures provide strong evidence that police enforcement can have a significant traffic safety impact, particularly when used in combination with a strong public information and education (PI&E) component. The amount of alcohol-crash reductions achievable by such countermeasures is difficult to quantify, but could be in the range of 10 to 30% for intense programs. Sobriety checkpoints appear to have been a successful enforcement strategy, both when used alone and when used in a DWI program that combined enforcement with PI&E that supports a specific strategy. Driver license suspension and revocation is clearly the most effective drunk-driving *sanction* currently known, particularly when it is well-publicized and applied administratively. However, this

effect seems to be due to most drivers not driving while the license is suspended or revoked, and reductions in alcohol-related accidents of up to 70% have been reported in the stronger studies. Studies of the effect of other severe sanctions such as mandatory jail terms have not yielded consistent results. Several studies of programs employing multiple countermeasures have given mixed results, some showing a positive effect and others no effect.

The findings of the most recent evaluations of *treatment and rehabilitation* countermeasures continue to indicate that they are ineffective or marginally effective in reducing alcohol-related crashes, although two strong studies found reductions in the re-arrest rate ranging from 10 to 35%. However, there is evidence to suggest that rehabilitative sanctions can enhance the effect of traditional sanctions such as driver's license suspension or revocation.

The past 10 years have shown an enormous increase in the number and quality of alcohol-traffic safety impact evaluations in the U.S. Many of these evaluations have been sponsored by state agencies through funds provided by NHTSA's 402 program. A number of others have been financed by funds appropriated by state legislatures. The state of California leads all others in the number and quality of their evaluations, and has also made it possible to obtain copies of their research. By contrast, some other states have either conducted no evaluations of alcohol-traffic safety programs, or could not identify any documentation of their evaluations. We strongly recommend that states interested in improving their evaluation program contact their nearest state university or the Chief of the Evaluation Staff, Traffic Safety Programs, National Highway Traffic Safety Administration, Washington, DC, 20590.

Despite this great increase in alcohol-crash countermeasure evaluation, some significant gaps remain. There is still a need for more rigorous evaluation of treatment and rehabilitation countermeasures. The recommendations of Mann *et al.*, 1988, should be taken into consideration in designing and executing these evaluations, particularly those recommendations pertaining to the use of control groups and random assignment of subjects, large sample sizes, and the pitfalls of using DWI recidivism as a measure of effectiveness. In fact, evaluations of other classes of countermeasures would also do well to consider these recommendations. The area of controlling the availability of alcohol by restricting alcohol sales or by increasing alcohol taxes has barely been touched in the evaluation literature. In view of the success of raising the legal minimum drinking age, this area needs attention. There is also a need for more evaluations of comprehensive alcohol-crash countermeasures, especially those operated on a community-wide basis.

In some areas we found no acceptable *impact* evaluations at all. Whether this is because none exists, or their results are not available, is not known. Some of these areas are:

- School-based educational interventions;
- Programs for developing responsible alcoholic beverage serving practices;
- Countermeasures for alcohol-impaired pedestrians;
- The deterrence value of economic sanctions (including fines) for DWI offenders;
- “Hard” versus restricted license suspensions and revocations;
- License plate and vehicle confiscation;
- Interlock devices;
- Server liability laws; and
- Lower legal limits for blood alcohol concentration.

NHTSA is currently sponsoring research projects in some of these areas, but more work remains to be done.

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NTS-33